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THE
DUBLIN JOURNAL

OF
MEDICAL SCIENCE;

EXHIBITING
A COMPREHENSIVE VIEW
OF THE
LATEST DISCOVERIES

IN
MEDICINE, SURGERY, AND THE COLLATERAL
SCIENCES.

VOL. XIII.

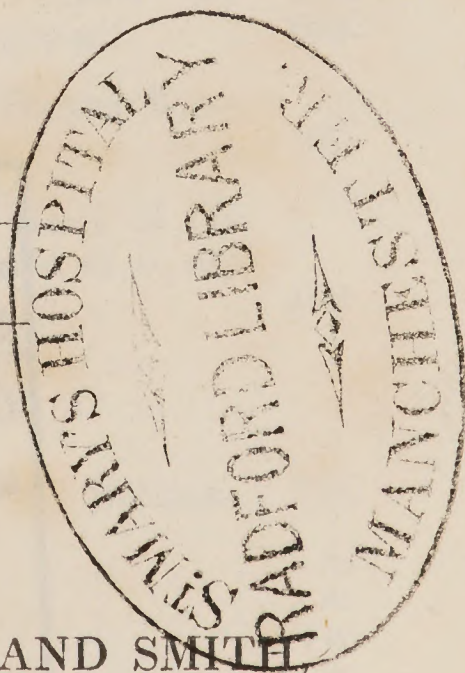
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CONTENTS OF No. XXXVII.

ORIGINAL COMMUNICATIONS.

	Page.
ART. I.—Cases of Acute Inflammation confined to the Epiglottis. By H. MARSH, M.D., M.R.I.A., one of the Physicians to Stevens's Hospital; Consulting Physician to the City of Dublin Hospital; and to St. Vincent's Hospital, &c. &c.	1
ART. II.—Journal of an Asthmatic	23
ART. III.—On some Effects resulting from Wounds of Nerves. By JOHN HAMILTON, L.R.C.S.	38
ART. IV.—The Medical Topography of Cove. By DAVID H. SCOTT, M.D., M.R.C.S., &c. &c.	55
ART. V.—Anatomy of Cowper's Glands in the Female. By J. R. TAYLOR, Assistant Surgeon, 58th Regiment	104
ART. VI.—Some Observations on Fever. By HENRY KENNEDY, A.B.	107
ART. VII.—Summary of Mr. CARMICHAEL's Views of the Venereal Cases in which Mercury is admissible or is not admissible	118

BIBLIOGRAPHIC NOTICES.

Gleanings from Foreign Medical Journals. By Dr. BIGGER	138
Gout in the Penis	ib.
Epilepsy cured by Indigo	ib.
Partus per Anum	140
Extraordinary Size of an Infant at Birth	ib.
Curative Effects of Nitrate of Silver in Nervous Diseases of the Stomach	ib.
Communication of the Poison of Rabies from one Human Being to another. By Dr. HAXTHAUSEN, District Physician at Neisse	142

	Page.
External Application of the Acid of Haller. By Dr. BRACH of Neustadt, District of Cologne	143
Incontinence of Urine cured by Strychnine	145
Inflammation of the Testicle treated by Compression	146
Oil of Turpentine in Neuralgia	147
Corrosive Sublimate in Syphilitic Pains in the Bones, in Rheumatic or Gouty Complications	148
Hydrargyrum Iodinicum in obstinate Syphilitic Swellings	ib.
Nitrate of Silver in Spasmodic Affections of the Heart	ib.
Application of Secale Cornutum to Cases of Hemorrhage	149
Pomade to cure Baldness	151
Delirium Tremens	152
Removal of Five Splinters of Iron from the Eye	153
Tinea Capitis	ib.
Practice of RICORD in Syphilitic Ulcers	ib.
On the Treatment of Syphilitic Buboës by Seton. By Professor LEVICAIRE, of the Marine Hospital, Lyons	155
Perforation of the Acetabulum caused by a fall on the Tro- chanter	ib.
Ossification of the Crystalline Lens	156
Numerical Tables made at the instance of the Academy of Paris, on the Influence of Climate on Phthisis. By MARC D'ESPINE	ib.
On the Proximate Cause, and Radical Cure of Varicose Veins of the Leg. By RIMA	157
Case of General Induration of the Skin. By Dr. FANTONETTI	158
Effects of Removal of the Ovaries from Cows	159
 The Cyclopædia of Anatomy and Physiology. Edited by ROBERT B. TODD, M.D.—Part XIII.	 159
An Essay on the Antiquity of Hindoo Medicine, including an Introductory Lecture to the Course of Materia Medica and Therapeutics, delivered at the King's College, Lon- don. By J. F. ROYLE, M.D., &c.	160
Dissertatio Inauguralis Medica de Typho Exanthematico, Halis Epidemio. Auctor JULIUS STABEROH, Berolinensis, 1834	ib.
Grundriss der Pathologischen Semiotik. Von A. F. SCHILL, Doctor der Medicin und Chirurgie, Privat Docentun an der Universitat zu Tubingen.	
A Plan of Pathological Signs. By A. F. SCHILL, Doctor of Medicine and Surgery, and Teacher at the University of Tubingen	161
Outlines of the Principal Diseases of Females, chiefly for the Use of Students. By FLEETWOOD CHURCHILL, M.D., Licentiate of the King and Queen's College of Physicians in Ireland; Physician to the Western Lying-in Hospital; Lecturer on Midwifery, &c., in the Richmond School of Medicine	162
A Lecture on the Nature and Cultivation of the Medical Profession, intended as a Guide to Students. By GEORGE T. MORGAN, A.M., Lecturer on Surgery, in Aberdeen	166

CONTENTS OF No. XXXVIII.

ORIGINAL COMMUNICATIONS.

	Page.
ART. VIII.—A Letter on the Metaphysical and Phrenological Opinions of Doctor Elliotson	169
ART. IX.—A Letter from PROFESSOR HAMILTON of Edinburgh, in reply to the Objections made to his Practical Precepts in Midwifery by DR. COLLINS	201
ART. X.—Second Medical Report of the Western Lying-in Hospital and Dispensary, 31, Arran-quay.—By FLEETWOOD CHURCHILL, M.D., Physician Accoucheur to the Hospital, and Lecturer on Midwifery, &c., at the Richmond Hospital School	233
ART. XI.—Report of the Newry Dispensary and Fever Hospital for the Year 1837. By J. MORRISON, M.D., M.R.C.S.L., Physician to the Newry Dispensary and Fever Hospital, &c.	249
ART. XII. —On Cirrhosis of the Lung.—By D. J. CORRIGAN, M.D., Lecturer on the Theory and Practice of Medicine; Physician to Jervis-street Hospital, and to the Fever Hos- pital, Cork-street; Attending Physician to the R. C. Col- lege, Maynooth; one of the Vice-Presidents of the Dublin Medico-Chirurgical Society, &c.	266
ART. XIII.—Observations on Sciatica and other Neuralgic Affections, with a Consideration of their frequent Origin from a disordered State of the Stomach; interspersed with some novel Views respecting the Origin and Prevention of Gall-Stones; both Diseases exemplified by the Author's own Case; together with an Account of the Waters of Bagnères de Bigorre and Barège in their Treatment. By RICHARD CARMICHAEL, M.R.I.A., Corresponding Member of the Royal Academy of Medicine of France, &c., and Consulting Surgeon of the Richmond Surgical Hospital, &c. &c.	286

BIBLIOGRAPHIC NOTICES.

	Page.
Sir DAVID BREWSTER's Theory of Cataracts	315
Practical Observations on the Pathology and Treatment of Typhus Fever. By ALBERT HENRY CALLANAN, M.D.	320
The Stomach in its morbid States; being a practical Inquiry into the Nature and Treatment of Diseases of that Organ; and into the Influence they exercise upon the Origin, Progress, and Termination of Diseases of the Liver, Heart, Lungs, and Brain. By LANGSTON PARKER, Member of the Royal College of Surgeons, and Fellow of the Royal Medical and Chirurgical Society of London, &c.	334
Philadelphia Medical Examiner	349

NOTICES TO CORRESPONDENTS.

The Editors beg to observe, with reference to the discussion between Mr. Faussett and the Editor of the Medico-Chirurgical Review, that they do not hold themselves accountable for the doctrines or expressions in any paper bearing the name of the author, which may appear in the Dublin Journal.—Dr. Johnson will perceive that the article in question is not an editorial one.

Dr. Johnson's able and dignified Reply shall be republished in our next Number.

It may be satisfactory to state, that Mr. Carmichael, while reading his paper on Neuralgic Diseases at the College of Physicians, produced the enlarged and scirrhus *Casserian ganglion*, alluded to at p. 299, as well as several other tumours of nerves.

Doctor Bulard (de Mèru) on the Plague, will appear in our next Number, arranged and digested by Doctor Bigger.

CONTENTS OF No. XXXIX.

ORIGINAL COMMUNICATIONS.

	Page.
ART. XIV.—On the State of the Pupil in Typhus, and the Use of Belladonna in certain Cases of Fever. By R. J. GRAVES, M.D.	351
ART. XV.—Statistical Report of the Richmond Lunatic Asylum. By JOHN MOLLAN, M.D., Physician Extraordinary to the Asylum, and one of the Physicians to the Whitworth Hospital, Drumcondra	367
Appendix to ditto	381
ART. XVI.—Some Observations on the Diagnosis of Cynanche Trachealis and Laryngitis. By TRAVERS R. BLACKLEY, A.B., M.R.I.A., Member of the Royal College of Surgeons in Ireland, and Medical Superintendant of the Hospital for Children, Portobello	384
ART. XVII.—Remarks on the Identity of Structure between the Kidneys and Epidermoid Glands. By ROBERT ELLIOTT LINDSAY	395
ART. XVIII.—Observations on Professor Hamilton's Deviations from the ordinary Mode of stating Practical Results. By ROBERT COLLINS, M.D., late Master of the Dublin Lying-in Hospital	402
Management of the First Stage of Labour	404
ART. XIX.—Researches on the Occurrence of Typhus in the manufacturing Cities of Great Britain, with some Observations on the Nature of its Enteric Complication. By JULIUS STABEROH, M.D., Berlin	426
ART. XX.—Cases. By DAVID H. SCOTT, M.D., M.R.C.S., &c.	442
Puerperal Mania, beneficial Effects of Belladonna	442
Ossification of Cerebral Veins	446
ART. XXI.—Letter from A. CARMICHAEL, Esq.	451

BIBLIOGRAPHIC NOTICES.

	Page.
Plague, from actual Observation. By Dr. BULARD, (de Méru.)	
Reviewed by Doctor BIGGER	459
Table of the Effects of Plague on the Population of Smyrna, for	
five months	463
Symptomatology	466
Crisis	469
Prognosis	ib.
Pathology	470
External Organs of Sensation	471
Muscular System	ib.
Nervous System	ib.
Lymphatic System	472
Pulmonary Apparatus	473
Circulating System	ib.
Digestive Organs	474
Organs of Secretion	475
Primary Lesions	476
Consecutive Lesions	479
Treatment	488
Conclusion.—Experimental Medicine	494
Immediate Contagion	495
Mediate Contagion	ib.
Prophylactics	499
Experiments and Observations on the Gastric Juice, and the Physiology of Digestion. By WM. BEAUMONT, M.D., &c.	
Reprinted from the Plattsburgh edition, by ANDREW COMBE, M.D., Fellow of the Royal College of Physicians of Edinburgh, &c.	500

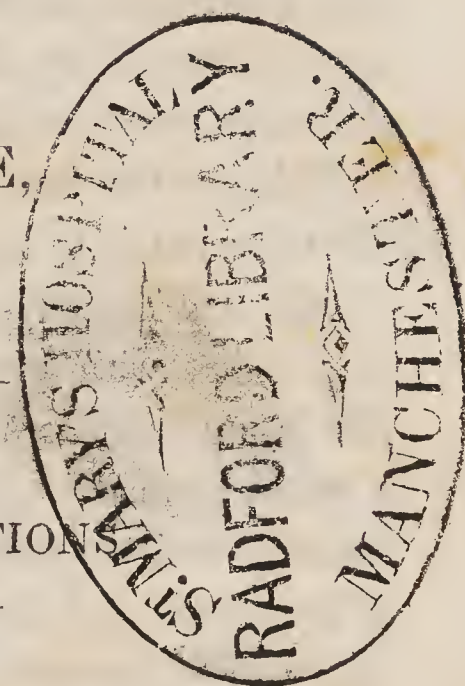
THE
DUBLIN JOURNAL

OF
MEDICAL SCIENCE,

1 MARCH, 1838.

PART I.

ORIGINAL COMMUNICATION



ART. I.—*Cases of Acute Inflammation confined to the Epiglottis.* By H. MARSH, M.D., M.R.I.A., one of the Physicians to Stevens' Hospital; Consulting Physician to the City of Dublin Hospital; and to St. Vincent's Hospital, &c. &c.

[Read at the Evening Meeting of the College of Physicians in the College Hall, on the 18th of December, 1837.]

M_{RS.} T——, between 50 and 60 years of age, robust, and of plethoric habit and florid complexion, had been confined for some days to her bed by a feverish cold. The symptoms having rather abruptly assumed a more serious aspect, I was requested to visit her, having been informed by the medical gentleman in attendance, that her case was an obscure one, and that he conceived her disease to be a nervous fever. I found her flushed, agitated, and complaining principally of being restless and sleepless; the moment her eyelids closed,

she started suddenly, gasped for breath, and looked like one about to be convulsed. She sat up erect in bed, and would not for a moment venture to lie down.

A continual accumulation of tenacious mucus obstructed the fauces, prevented sleep, and produced frequent and urgent paroxysms of dyspnœa ; her skin was hot and dry ; her pulse 120, contracted and resisting ; and to the inquiry whether she were distressed by thirst, she replied, she was afraid to drink, the attempt to swallow was productive of so much pain and suffering. On handing her a glass of water, she seemed to dread to put it to her lips.

On making an effort to swallow, a struggle so painful and convulsive, with protrusion of the eyes, ensued, that it gave rise in my mind to the suspicion that she had been bitten by a rabid animal. In attempting to swallow, the fluid was forcibly and convulsively rejected. On examining the interior of the mouth and fauces, I could discover no appearance indicative of inflammation or disease of those parts ; the tonsils, uvula, and soft palate, presented a perfectly natural appearance, but the forcible depression of the tongue produced so much pain, and caused such convulsive movement of the muscles, that I was unable to obtain a satisfactory view of the parts which lie more posteriorly ; the tone of her voice was scarcely, if at all, altered : the respiration in the intervals between the paroxysms was unimpeded, and it was not at any time in the least degree stridulous.

This case fell under my observation early in my professional life, and I felt very much at a loss to account for the symptoms ; it occurred to me at the moment to pass my finger along the tongue, and endeavour to ascertain by the touch the cause of the symptoms. On passing the finger over the tongue, as far towards its root as I could reach, I felt a tense, rounded, prominent body, which, upon close examination, was evidently the epiglottis, considerably enlarged and erect. This at once revealed the seat of the disease, and the source of all the dis-

troubling symptoms ; I had not previously witnessed an instance of this disease, nor had I in the course of my reading met with a description of it. My opinion was at once formed—it was now quite certain that I had to do with a case of acute inflammation of the epiglottis, accompanied with high inflammatory fever.

The treatment was obvious : the patient was instantly and largely bled from the arm, and leeches numerous and successively applied, so as to maintain for some time a continual flow of blood ; the thick tenacious mucus was removed from the fauces from time to time, by means of a sponge attached to a piece of whale bone. After a few hours the symptoms were considerably abated, and some rest was obtained ; bleedings from the arm were repeated a second and a third time, and each repetition of the bleeding was followed by a still more evident abatement of the symptoms ; she was now able, though with considerable pain and difficulty, to swallow. Calomel and Dover's powder were given in repeated doses, and the symptoms having continued for some hours stationary, it was determined to introduce mercury by friction, so as to influence the system as speedily as possible. Half a drachm of mercurial ointment was rubbed in every fourth hour ; moderate ptyalism was established ; the remaining symptoms rapidly subsided, and after the lapse of a few days, scarcely a trace of the disease remained.

This lady has since enjoyed perfect health, no disposition to a return of the disease having manifested itself.

The next very remarkable instance of this affection which fell under my observation, occurred in an individual, whose constitution and temperament were altogether the reverse of those which characterized the case just related. The disease manifested itself in a young person not twenty years of age, of delicate frame, and feeble constitution, and arose in the following manner :

In consequence of exposure to cold, this young lady was

affected with slight rigors; fever ensued, accompanied with some tumefaction and redness of the tonsils and uvula. The fever and inflammation yielded to the quiet of bed and to diaphoretic medicines. On the third day she was convalescent.

On the evening of the third day, contrary to the injunctions she had received, she got up and exerted herself; on returning to bed she felt herself chilly and uncomfortable, and spent a feverish and restless night.

Next morning she experienced some pain and difficulty in deglutition, and these symptoms continuing rapidly to increase during the day, I received an urgent message to see her. On visiting her in the evening, I found her in a state of great suffering and distress; she was incessantly harassed with a profusion of tenacious transparent matter, which was generated about the fauces, and every effort to rid herself of this was attended with very great distress; the act of protruding the tongue was extremely painful to her; she feared to lie down; there were frequent paroxysms of dyspnœa; she was agitated and apprehensive to an extraordinary degree; her friends around her were in a state of the greatest alarm; every effort to swallow was accompanied with violent pain, the liquid was rejected with force through the nostrils, and her distress and agitation were much increased. On examining the fauces there was scarcely a remaining blush, and the tumefaction which had existed on the first attack had entirely subsided; it was impossible sufficiently to depress the tongue to obtain a view more posteriorly, but on introducing the finger, the epiglottis was found greatly swollen, tense and smooth, and pressure with the finger produced severe pain; there was no external swelling; she experienced some uneasiness on pressure upwards at the highest part of the neck; the face was darkly flushed, anxiety and suffering were strongly depicted on the countenance, and the pulse was incalculably frequent, small, and feeble; the skin was hot, and covered with a clammy perspiration; the

voice was natural; there was no cough, but the efforts to clear away the phlegm from the throat were almost unceasing; the respiratory and percutatory sounds were perfectly natural. Leeches were applied externally, as near as possible to the seat of the disease, and fomentations with the decoction of poppies, after the removal of the leeches, perseveringly employed.

I visited her again very late at night, accompanied by Mr. Cusack, whose cooperation I was requested to obtain. There was no abatement, rather an increase of all the urgent symptoms. Mr. Cusack, by sewing a dossil of lint to the end of the finger of a glove, and putting it firmly on his fore-finger, was enabled to remove a large quantity of the adhesive mucus from the fauces, and thus procure considerable temporary ease; a short interval of sleep in the erect position was thus obtained; leeches were again applied, and fomentations repeated. No medicine could be given by the mouth; purgative injections were administered, and acted well.

At our next visit, though no positive abatement of the symptoms could be said to have taken place, yet the patient on the whole seemed somewhat less agitated and distressed; the fauces were again cleansed in the same manner as before. It was now determined in consultation to apply, by means of the dossil of lint attached to the finger of a glove, a solution of the nitrate of silver (ten grains to an ounce of distilled water) to the inflamed epiglottis. At first some pain, afterwards marked relief, ensued from this application, which was repeated at every subsequent visit, the strength of the solution being gradually increased; and it appeared to me that its employment was always attended with benefit. We also determined to commence at once mercurial treatment, the urgency of the symptoms demanding its immediate application; a drachm of mercurial ointment was accordingly ordered to be rubbed in on the inside of the thighs, every fourth hour, and in addition, to excite as speedily as possible mercurial action in the system, the

surface of the body under the bed-clothes was exposed to the continual contact of volatilized mercury; the bed-clothes being retained in an elevated position, this was readily accomplished; the vapour arising from the hydrargyrum cum creta heated, was generated abundantly and retained between the bed-coverings; and by perseverance in this, the entire surface of the patient's body was kept constantly involved in a mercurial atmosphere.

On the third day from the commencement of this treatment, moderate, but decided ptyalism was established; the occurrence of which was coincident with a marked and rapid abatement of every distressing symptom. Previously to the constitution having been placed under the influence of mercury, a mitigation of the symptoms had been effected. On the third day the patient was able, though with great pain, to swallow small quantities of fluids, and short, but refreshing intervals of sleep were procured. It was not however until the end of the fifth day, when the decided action of mercury on the system became apparent, that a complete and permanent subsidence of the symptoms was manifested.

After this period deglutition became easy, the paroxysms of dyspnœa no longer recurred, and the sleep was tranquil and refreshing.

Towards the end of the third day the epiglottis felt rough to the touch, but still swollen and large: afterwards the feel was that of a body that was wrinkled and puckered, but not much swollen. After the lapse of many days, I examined again the epiglottis, and the touch could detect no abnormal condition of this organ. Convalescence was slow and tedious.

The next case was that of a man aged 40, who after exposure to cold, was affected with much pain and difficulty of swallowing, the liquids regurgitating through the nose in every effort. On looking into the mouth, the tonsils, soft palate, and uvula presented a natural appearance, but at the base of the tongue was seen a round, red, prominent substance, like a small

ripe cherry. The attendant fever was considerable. For three days he was unable to swallow any thing: he was relieved by frequent relays of leeches; warm baths, and anodyne enemata having been used without apparent benefit.

The constant oozing of blood from the leech-bites appeared to be the only effectual part of the treatment. On the third day the inflammation began gradually to subside, and the patient ultimately, but slowly, recovered.

The anatomical characters and relations of the epiglottis fully explain the nature and course of the symptoms detailed. Being studded with glands at its root, the inflammatory irritation stimulates them to a greatly increased secretion of mucus: this adhesive mucus, in constant efforts to detach it, greatly harasses the patient, produces paroxysms of dyspnoea, and prevents sleep. The connexion of the epiglottis with the root of the tongue accounts for the pain felt, when the tongue is moved or protruded, and its situation and the relation which it bears to the muscles of deglutition, fully account for its essential symptoms, pain, spasm, and difficulty in the act of swallowing.

The loose attachment of the mucous membrane to its anterior surface will explain satisfactorily the great extent of the tumefaction, and also that the inflammatory distention should be situated on the lingual rather than on the laryngeal aspect of the organ, as well as for the puckered or wrinkled feel of the mucous membrane on the absorption of the effused fluid.

I shall briefly refer to the only cases of pure unmixed epiglottitis which I have been able to find recorded: they are well marked examples of this disease; the symptoms, however, are imperfectly detailed.

In the first vol. of *Medical Facts and Observations* for the year 1791, a case is related by Mr. Mainwaring of a gentleman about 40 years of age, who having been exposed to the influence of cold during the day, was attacked in the night with a violent pain in his throat and a total inability to swallow. The next morning the symptoms were much increased; the pain was not

in the situation usual in similar affections, but lower down, and felt more anteriorly. When he attempted to swallow fluids, they passed readily to the root of the tongue, where they were not allowed to remain for a moment, but were immediately forced out of the mouth with considerable violence. Upon examining the throat, the tonsils, as well as the palatum molle and uvula were in a natural state, having no tumefaction, nor were they even materially redder than common, so that in this view of the parts, there was no appearance of disease, but upon pulling the tongue forwards, and looking down into the throat, the epiglottis was immediately brought into view in a very unnatural state, and with a very unusual appearance: it was much swelled, extremely red, and looked by no means unlike the glans penis when distended with blood and in its erected state. It stood directly up, so that nothing could pass over it, and there was very little room laterally between it and the sides of the pharynx.

All the other parts were apparently free from disease. Leeches and a blister were applied externally to the throat, but were followed by scarcely any abatement of the symptoms. The complaint continued with little or no diminution until the fourth day, when the swelling or more properly the sensibility of the epiglottis was so far reduced as to allow the patient to swallow small quantities of fluids; and by the seventh day he could with some pain and difficulty take solid food.

Mr. Manwaring considers this affection of the epiglottis when entirely independent of disease in the neighbouring parts, as one of very uncommon occurrence. The mode of treatment particularly as it did not appear to be very efficacious, has been in a great measure passed over. In the third vol. of the *Transactions of a Society for the Improvement of Medical and Surgical Knowledge*, 1808, Sir Everard Home relates three cases in which the epiglottis had become enlarged in consequence of inflammation, while the tonsils and other neighbouring parts were nearly in a natural state: this appearance, he adds,

was of such rare occurrence, that none of his medical friends had met with it. The first instance occurred in a man 40 years of age, rather short, of a stout make, with a tendency to corpulency, and of sedentary habits, who had been exposed to rain, during a very tempestuous evening. In the course of the night, while in bed, he felt very uneasy in his throat, and was unable to swallow. The uneasiness increased, and, in the morning, every thing he attempted to swallow was rejected with considerable force. On examining the throat, there was no appearance of swelling in the tonsils, nor of inflammation of the palatum molle or uvula. These parts were not even more red than they usually are, but immediately beyond the root of the tongue there was a rounded, projecting substance, of a bright red colour, bearing a very close resemblance to the glans penis, in its distended state.

At first I was at a loss to know what this could be, but on examining it more narrowly, I found that it was the epiglottis much enlarged, the membrane covering it being inflamed and thickened. It continued for three days in this state, and the patient could not swallow during that period; but there was no sensation of thirst. Leeches were applied to the outside of the throat, and immediately gave some relief. The warm bath was used, but without apparent advantage. Dover's powder was administered by clyster, but it did not bring on perspiration, or soothe the distressing symptoms. The inflammation subsided gradually on the fourth day; he could swallow fluids when his thirst became excessive. On the seventh day he could take his food as usual. During the whole of the attack his breathing was not at all affected. The parts recovered so entirely, that he never afterwards had any return of the complaint, or uneasiness in the part which had been affected.

The other cases were so exactly similar to this, that it is unnecessary to detail them.

The following cases of acute inflammation of the epiglottis,
VOL. XIII. NO. 37. c

by Dr. Burne, were communicated and read at the College of Physicians in London by Dr. Hawkins.*

The author observed, that acute inflammation affecting the epiglottis without extending to the contiguous parts, is so rare a disease that few examples of it are to be found on record. Two cases of this nature, however, had fallen under Dr. Burne's notice. The first patient recovered, but the second perished at the end of four days. In both, the most remarkable symptoms consisted in the extreme difficulty or impossibility of swallowing, while the throat did not exhibit any appearance of tumefaction.

A journeyman, aged about fifty, at the time he was seen by the author, had been labouring under difficulty of swallowing for thirty hours. So great was the impediment to deglutition, that not a drop of water was allowed to pass. On attempting to look into the throat, great difficulty was experienced, but this having been, by some perseverance, overcome, a satisfactory view of the parts was at length obtained. The fauces were open and unobstructed, having a diffuse redness over them. Anxious to ascertain what might be the cause of the difficulty of deglutition, the tongue was forcibly depressed, when the epiglottis came into view—prominent, red, swollen, and resembling a Kentish cherry. The complaint was nearly local, consisting in extreme difficulty of swallowing, and in the extreme exhaustion dependent thereon.

The system generally sympathized but little—there was inflammatory fever indeed, but it was mild. He had been already bled to syncope; leeches had been applied to the throat, and he had been purged. Leeches were again applied, and tartar emetic given in pills, while mercurial ointment was rubbed in, to guard against the probable consequence of infiltration about the glottis. This treatment brought the patient up to the fourth day, when the fever and inflammation had subsided; but the epiglottis continued so much swollen, as to render the introduction

* London Medical Gazette, vol. vi. p. 313.

of nourishment in any form extremely difficult, though it now became strongly indicated by the state of exhaustion, and by the return of the appetite. Next day he was able to swallow fluids, and ultimately did well.

The second case occurred in a lady far advanced in pregnancy, who was attacked with the usual symptoms of cold and sore throat, to which was added tenderness about the larynx discoverable by pressure made externally.

Next day deglutition was almost impossible, yet unattended by any tumefaction of the fauces. Bleeding was had recourse to, both generally and locally, but the issue was unfavourable. On examination the epiglottis was found stiff and thickened, with traces of pus, the surrounding parts of the larynx were sound; the lungs unfiltrated with serous effusion. Dr. Burne thinks that blood-letting from the jugular vein, as being in the neighbourhood of the inflammation, would probably be the most eligible mode of depletion. To this may be added purgative enemata, mercurial inunction, and blisters to the throat. Dr. Chalmers and Mr. Stanley, who also attended the second case, recommended scarification of the epiglottis, as likely to diminish the tumefaction of the part. The symptoms were not such as to require tracheotomy.

My friend, Dr. Fleming, has favoured me with the particulars of a case of acute œdema of the lingual surface of the epiglottis. The patient had been under treatment for secondary syphilis, when he was attacked with uneasiness in the throat and pain on deglutition. The fauces and pharynx appeared healthy, nor did the finger discover any lesion of the superior border of the epiglottis; but its lingual surface was thickened and painful. There was no laryngeal symptom. In the course of two days fever supervened, and the patient experienced a most distressing sense of choking, accompanied by paroxysms of dyspnœa so severe as to threaten suffocation. The voice continued perfect; there was no stridor, and in the intervals of the attacks the respiration was tranquil. The paroxysms of dyspnœa were

brought on whenever the patient attempted to lie down. He suffered much from thirst, which he could not gratify in consequence of the drinks being rejected through the nostrils.

On examining by the finger, the base of the tongue was found painful, and giving the sensation of two distinct tumours, separated by the frænum of the epiglottis. The disease gradually subsided under repeated leeching, fomentations, blisters, &c. For some time the lingual portion of the epiglottis remained in a state of chronic œdema.

OBSERVATIONS.

In the cases, the facts of which have been just detailed, the inflammatory action was limited in extent ; not extending probably beyond the anterior surface of the epiglottis. The circumscription of the inflammation was marked by positive as well as negative signs. The inspection of the fauces proved that it did not exist in the parts anterior to the epiglottis—the absence of dyspnoea except in paroxysms ; of stridulous breathing—the natural tones of the voice, when the mucous matters were detached—the exemption from cough—the result of stethoscopic examination ; all these negative signs prove that neither the glottis, larynx, nor bronchial mucous membrane, shared in the inflammatory action. These cases are therefore examples of unmixed, circumscribed, inflammation of the epiglottis.

I am inclined to think that the disease termed *cynanche pharyngea*, described in systematic works, is but an example of this disease ; the examination, however, of the epiglottis, either by inspection or by the touch, having been neglected, the true seat of the disease was overlooked.

Numerous and interesting cases of inflammations of the epiglottis, both acute and chronic, are upon record, several in the periodical publications ; some of the most valuable of these are to be found in Mr. Porter's excellent work on the *Surgical Pathology of the Larynx and Trachea*, and also in Dr. Stokes's lately

published and valuable work on the Diseases of the Chest, but in all these the disease of the epiglottis forms but a link in the united chain of symptoms. It is associated with morbid action, extending either from above downwards, or from below upwards, engaging either the tonsils and soft palate, or, as most frequently happens, the glottis and larynx.

Slight cases of this disease are not unfrequent, but require little or no treatment. In cases of medium severity, the symptoms are not very urgent, and will, with mild antiphlogistic treatment, subside ; but in the more acute and intense examples of this disease, the urgency of the symptoms and the sufferings of the patient demand the most prompt and vigorous treatment. From the cases I have observed, and from those recorded, I am disposed to gather that the disease has a tendency to abate either on the third, fifth, or seventh days, but if not combated by energetic remedies, it may end in suppuration, as it did in one recorded case, or it may extend downwards, involve the glottis, and thus produce a still more dangerous disease, œdema of the glottis ; or by causing repeated paroxysms of dyspnoea, it may give rise to pulmonary infiltration ; or it may but partially subside, and leave behind a thickened, indurated, and permanently diseased condition of the epiglottis. To allay the urgent symptoms, and prevent these consequences, active treatment, regulated of course by the patient's constitution, is imperatively required. In one of the cases I have recorded, frequent and large bleedings, general and topical, with the active administration of mercury, were necessary to reduce both the concomitant fever and the local inflammation. In another, the delicate constitution, the feeble pulse, the state of the skin, induced me to restrict bleeding to the reiterated application of leeches as near as possible to the seat of the disease. To subdue inflammatory action, as well as to prevent lingering chronic disease, mercury is invaluable. Fortunately it can be applied as effectually, and I think as rapidly, by the skin, as when administered internally. In cases such as those

now detailed, "where the ability to swallow is lost, its external application, both by friction and by vapour, is of immense value, and it seems to me that in the more severe cases of the disease, this part of the treatment should not be omitted. I think the application of the nitrate of silver, as suggested and practised by Mr. Cusack, was decidedly useful. Fomentations long continued, though a minor remedy, are not without their value. Blistering I did not think necessary, nor would I apply a blister till the inflammatory excitement were markedly reduced.

From the administration of tartar emetic, at the period of commencing restoration of the power of deglutition, I abstained, fearful of exciting vomiting, which would, I conceive, be distressing, perhaps dangerous to the patient. Were, however, the fever to continue, and the symptoms not to yield satisfactorily, it might be given, guarded with opium in carefully regulated doses. The influence of this combination of medicine, in diminishing excitement, and reducing inflammation, is established incontestibly in various conditions of disease. But the symptoms, in the cases recorded, yielded so completely to bleeding, mercury, and fomentations, that I did not deem it necessary to resort to other means, and in none of them has any trace of chronic disease been left behind. Now, to contrast the effects of intense inflammation with those resulting from the actual loss of the organ, I shall give a highly interesting case recorded by Baron Larey, of the excision, if I may so express it, of the epiglottis, by a gun-shot wound.

"A soldier was wounded at the battle of Abon-qyr ; the ball entered at the angle of the jaw, crossed the throat obliquely, and came out at the jugular region on the opposite side. The back part of the tongue was furrowed, and the epiglottis carried away. The wounded man spat it up after the accident, and shewed it to the surgeon who had first rendered him assistance. The patient suffered little, but his voice could only be heard with difficulty, in a hoarse and very feeble manner. When he attempted to swallow, for the first time, he got a convulsive and suffocative

fit of coughing, accompanied with vomiting. He was tormented by thirst, which the excessive heat of the season and the irritation of the wound produced.

“He experienced already violent pain in the stomach, and the inability to sleep was continual ; the pulse weak and rapid, and wasting of the flesh began to be manifest. After having endeavoured to make him drink, I was convinced the cause of the suffocations and the impossibility of swallowing depended on the permanent patency of the glottis, the valve of which had been carried away by the ball : a singular, and I believe, unprecedented accident. The indications were not less difficult to fulfil.

“The most urgent was to appease the hunger and thirst which tormented this honourable victim. I was most fortunately provided with a gum-elastic tube, which I introduced with the necessary precautions into the pharynx, and, by the aid of this, I got the patient to swallow. This method, long-continued, saved the life of the soldier, but the difficulty of swallowing continued, and speech was not restored till after a considerable lapse of time, and still in a very imperfect manner. At about the end of six months he could swallow without the tube, and on his departure for France, he could eat rice, which he prepared in the form of little balls.

“The functions of speech and deglutition have been in time made perfect, no doubt because the arytenoid cartilages will, in part, supply the place of the epiglottis by their development.”

Thus we see that very similar symptoms are produced by intense inflammation, which tends to impair or annihilate function, and the total removal of the organ.

I have never met with an example of the acute, nor indeed of the chronic form of this disease, arising from the same causes as in adults, in very young children ; in them, however, it is not unfrequently produced, if I may so say, artificially.

It is produced by an accident to which the children of the poor are liable. Slatternly mothers often permit their children

to drink from the spout of a tea-kettle, or tea-pot, and they are thus led to a habit, which, if the kettle should happen to contain boiling-water, may prove fatal. The symptoms produced by this accident are those which characterize inflammation of the fauces, epiglottis, and glottis, but not usually extending into the trachea. In the third volume of the *Dublin Hospital Reports*, there is a case given by Dr. Burgess of a girl of three years of age, who drank boiling-water from the spout of a tea-kettle, from which great swelling of the parts immediately ensued, thereby preventing deglutition, and impeding respiration.

In about two hours after the accident, on looking into the mouth, it appeared as if a large piece of raw flesh had been forced into the fauces, and had completely filled up the passage; respiration was performed with great difficulty, and was rapidly becoming more laborious; in fact the child appeared to be dying, when bronchotomy was performed, by which life was saved.

I asked Mr. Porter what the results were of his observations in the many cases of this accident which have fallen under his cognizance; his reply was, that in every instance the epiglottis was to a greater or less degree inflamed. From the nature of the accident, such is the result which might be anticipated.

Causes, such as chills of cold, and exposure to wet and damp, which in adults are productive of inflammation of the epiglottis, and of the glottis and larynx; an inflammation from which effusion into the submucous cellular tissue results—in young children produces a disease similar in its effects, but totally different in its results. They are affected with croup.

The inflammatory action, either as happens in a few instances, confined to the larynx and a few of the rings of the trachea, or as most frequently happens, extending itself along the whole of the mucous membrane of the bronchial ramifications, issues in effusion; not as in adults, under the mucous membrane, but upon its external surface, thus forming the adventitious membrane which obstructs the air passage, and

forms one of the most remarkable pathological conditions of croup.

That the inflammatory deposition should occur *under* the mucous membrane in adults, and *upon* it in children, is a very curious fact, and will help to explain the infrequency of the disease of which we now treat, in very young children. I remember having been acquainted many years ago with a very intelligent German physician, who informed me he had tried a series of experiments on this subject; his object was to produce croup in animals; his experiments were made on dogs; by opening the trachea and applying irritants, (of which he found the tincture of iodine the most certain,) he succeeded in producing the symptoms and effects of croup; but as he particularly stated, the adventitious membrane he could not cause in full grown dogs, it was only in very young ones that this effect resulted from the artificially excited inflammation, and the younger the more certain. This gentleman has since published the results of his experiments. Should then the epiglottis become the exclusive seat of inflammation in very young children, we should rather expect a layer of lymph, forming on its external surface an adventitious membrane, than the sub-mucous effusion we find in adults.

It is of some consequence to remark, that in almost every case of acute laryngitis, indeed, so far as my own recollection leads, I would say in every case, the epiglottis partakes in the inflammatory action.

I shall quote from the short, but beautifully written treatise on Laryngitis, inserted in the *Cyclopædia of Medicine*, the words of the late lamented Dr. Cheyne: "That laryngitis is an inflammatory affection, we have abundant proof; the fever which belongs to the disease is attended with increased heat of surface; frequent and strong pulse; the blood is sisy; the parts affected are swelled and painful: and we sometimes obtain a view of a portion of the affected part in a state of intense inflammation.

“When the tongue is not much swollen, by depressing its root downwards and forwards by means of a spatula, we can discover the epiglottis, erect, florid, rounded and swelled. One writer on the disease has well described the epiglottis, as being enlarged, red, glossy, and nearly of the size and appearance of a plumb.

“Inflammation, thus denoted by swelling and glossy redness, is doubtless the state of the glottis as well as of the epiglottis.”

Dr. Cheyne adds in another place :—

“If we had any certain means of ascertaining when the membrane is merely inflamed, and when it is œdematous as well as inflamed, much of the difficulty which attends the treatment of the disease would be removed.”

Now, what do I deduce from these quotations? the valuable fact, that the epiglottis being generally within the reach of vision, always of touch, we are enabled to judge of its real state; we can know whether or not it is œdematous; we can thence deduce and judge of the state of the glottis; and this very investigation might decide the important and difficult point in practice, whether or not the time is arrived when the operation of tracheotomy should no longer be postponed.

It might be the very means of determining the immediate performance of an operation, which, if delayed until the functions of the brain be overthrown, can have no other issue than to add to the list already too long of unsuccessful operation—unsuccessful because postponed to too late a period. I have long been in the habit of instructing the pupils at Stevens’s Hospital to educate the eye and the finger, that they may be enabled, from experience, both by touch and sight, to form a correct judgment of the state of the epiglottis; as a practical guide in cases of laryngitis this is of considerable value.

As it may be interesting to some of my hearers, and as the case bears upon our subject, I shall here introduce the first recorded case of laryngitis—the disease which terminated the

earthly career of one of the most conspicuous and one of the most truly great men who ever lived, General Washington.

“ Some time on the night of Friday, the 10th of December, 1799, having been exposed to rain on the preceding day, General Washington was attacked with an inflammatory affection of the upper part of the wind-pipe, called in technical language, *cynanche trachealis*. The disease commenced with a violent ague, accompanied with some pain in the upper and fore part of the throat, a sense of stricture in the same part, a cough, and a difficult rather than a painful deglutition, which were soon succeeded by fever and a quick and laborious respiration. The necessity of blood-letting suggested itself to the General; he procured a bleeder in the neighbourhood, who took from his arm in the night, twelve or fourteen ounces of blood. He could not by any means be prevailed on by the family to send for the attending physician till the following morning, who arrived at Mount Vernon at about eleven o'clock on Saturday. Discovering the case to be highly alarming, and foreseeing the fatal tendency of the disease, two consulting physicians were immediately sent for, who arrived, one at half-past three, and the other at four o'clock in the afternoon. In the meantime were employed two pretty copious bleedings, a blister was applied to the part affected, two moderate doses of calomel were given, and an injection was administered which operated on the lower intestines, but all without any perceptible advantage, the respiration becoming still more difficult and distressing. Upon the arrival of the first of the consulting physicians, it was agreed, as there were yet no signs of accumulation in the bronchial vessels of the lungs, to try the result of another bleeding, when about thirty-two ounces of blood were drawn without the smallest apparent alleviation of the disease. Vapours of vinegar and water were frequently inhaled; ten grains of calomel were given, succeeded by repeated doses of emetic tartar, amounting in all to about five or six grains, with no other effect than a copious discharge from the bowels. The powers of life

seemed now manifestly yielding to the force of the disorder: blisters were applied to the extremities, together with a cataplasm of bran and vinegar to the throat. Speaking, which was painful from the beginning of his complaint, as well as through every succeeding stage of it, now became almost impracticable; respiration grew more and more contracted and imperfect till half-past eleven on Saturday night, when, retaining the full possession of his intellect, he expired without a struggle.

“He was fully impressed from the beginning of his complaint, that its conclusion would be mortal. He considered the operations of death upon his system as coeval with the disease; and several hours before his death, after repeated efforts to be understood, succeeded in expressing a desire that he might be permitted to die without further interruption. During the short period of his illness, he economized his time in the arrangement of such few concerns as required his attention, with the utmost serenity, and anticipated his approaching dissolution with every demonstration of that equanimity for which his whole life had been so uniformly conspicuous.”

Such were the last moments of the life of this great man, whose love of country always rose far superior to his love of self; who, in the midst of difficulties, and thwartings, and unjust aspersions, never swerved from his steady course, never lost sight of his one great object—the liberation of his country; who, surrounded by obstacles which would have borne down any other mind than his, led his ill-equipped armies to victory, and having filled the cup of glory, retired contentedly—without even aiming at personal aggrandizement—to his home, his house, to his sequestered and unostentatious agricultural pursuits. The same calmness and strength of mind which marked the course of his eventful life, did not forsake him as it approached its close. Whilst to history belongs the deeds of a man, the rarest of his kind, who was in truth a patriot, to us belongs the interesting document which records the disease, then supposed a new one, now well known, which closed the earthly

existence of this remarkable man. The early performance of the operation of bronchotomy might have saved General Washington's life.

A very interesting and curious case published in the 5th vol. of the *Dublin Hospital Reports*, I shall here introduce.

Case of Attempt at Suicide, with Danger of Suffocation by the falling down of the Epiglottis. By Dr. Houston.

“ In March, 1828, a servant out of place, residing in Duke-street, during a fit of delirium brought on by intoxication, attempted to destroy himself by cutting his throat with a razor. I saw him in about ten minutes after, and found him almost lifeless. The pulsations of the heart were almost imperceptible; the pulse at the wrist had ceased to beat; the limbs were cold, and all feeling and consciousness lost. The wound, which was frightfully deep, extended more towards the left than the right ear; the razor had entered between the os hyoides and the thyroid cartilage, and disunited them so completely, as to allow the former to ascend with the tongue into the mouth.

“ The pharynx was laid wide open, and the epiglottis severed from its attachment to the tongue and os hyoides, and left hanging by its pedicle to the back of the pomum Adami; the carotids had remained untouched, and the bleeding was inconsiderable. It appeared difficult at the moment to account for the sudden extinction of life; the symptoms were evidently those of suffocation, but the cause was not at first understood.

“ I passed my finger into the wound, and found to my surprise, that the epiglottis, loosened from its upper and lateral attachments, had fallen back over the rima glottidis, and completely intercepted the passage of air to the lungs. I raised the obstructing body and drew it forwards; the chest soon afterwards began to heave; respiration returned; the heart and pulse again beat, and consciousness and sensibility were re-established.

“ It required some effort of my fingers to hold up the epi-

glottis, as the air at every inspiration tended to force it back again to its natural position.

“ While thus occupied with the patient Mr. Porter entered the room ; I explained to him the singular nature of the case, and how between my finger and thumb I held the regulation of a man’s life or death. The top of the epiglottis was then brought over the edge of the thyroid cartilage and secured to its anterior surface by a single stitch.

“ The man in a short time sat up and attempted to speak, but was unable to articulate.

“ He was taken into the Meath Hospital, under the care of Mr. Porter, from whom I learned that he never recovered from the delirium which led to the perpetration of the act, and died in about a week after of erysipelatous inflammation of the neck and throat.”

We have not on record any instance of congenital mal-formation of the epiglottis. Mr. Barker mentioned to me that a child about fifteen months old, died suddenly whilst playing and laughing : that whenever this child laughed, she emitted a peculiar sound ; and he stated that on examination of the body by the late Dr. Andrew Johnson, he found the epiglottis malformed and elongated ; that the elongated portion plugged the aperture of the glottis, and thus caused immediate suffocation.

I have stated the fact as reported to me by Mr. Barker : I do so chiefly with the view of exciting attention to the subject of mal-formations of the epiglottis : future observations may bring to light points not heretofore ascertained.

My chief object in this paper has been to dwell upon the symptoms and treatment of acute inflammation of the epiglottis—a disease which, though often alluded to, has not hitherto been accurately described ; and thus to supply a deficiency in the history of the symptoms, progress, pathology, and treatment of disease. This, from having witnessed some severe cases of the

disease, I am enabled, in some measure, to do. Accumulating facts will render the history more complete. I have not touched upon the chronic diseases of this organ. It may be hypertrophied, or atrophied, or ulcerated, or utterly destroyed by ulceration, (of which Mr. Reid furnished me with an interesting example,) and these effects may arise from neglected inflammations or from scrofulous, or syphilitic, or carcinomatous causes ; but in every chronic case, the disease is not confined to the epiglottis: the neighbouring parts partake more or less in the morbid degeneration. Were I to pursue the subject further, it would lead me into a length of detail, quite unsuitable to the present occasion. I shall then conclude my observations by an expression of the pleasure it gives me, who am now a veteran in the service, to observe the zeal with which the science of medicine is cultivated in this city. Not in any city in Great Britain—I might say in Europe—is it prosecuted with more ardour and success.

This very assembly, these meetings in this Hall, now commencing for the season, have tended to give an additional impulse to our exertions. The *vis inertiae* is almost as much an attribute of mind as it is of matter, and every stimulus applied to set the mind in motion and propel its energies, is calculated to augment the amount of human enjoyment, and in our pursuits especially to enlarge the sphere of usefulness, and to place more and more within our reach the application of those means largely provided by creative wisdom which are designed to alleviate suffering and to prevent and stem the destructive progress of disease.

ART. II.—*Journal of an Asthmatic.*

It may be necessary to state, that the following diary was kept at the request of one of the Editors of this Journal. The gentleman who is the victim of the disease which he so well de-

scribes, has been for many years subject to spasmodic asthma. The period embraced in the diary is about four months, during which time Mr. A. has had many fits of his terrible disease, all of which he describes in a most instructive and interesting manner. Among the more remarkable points in this history of suffering, we would refer to the cerebral sympathies which are peculiarly well marked in Mr. A.'s case.

Saturday, 7th February. Symptoms of an approaching fit began to appear at four, P.M. The principal were fulness in the head, dulness and heaviness of the eyes, and disagreeable drowsiness. The drowsiness increased so much that I spent a great part of the evening in a succession of "trances," as I shall call them. I felt so chilly that I sat quite close to the fire. This horrid drowsiness generally prevents me from being sensible of the approach of a fit till it has commenced. In the present instance I was not so sure of the approach of the fit as to prevent my mattress (which is my bed) from being prepared. At twelve, being unable to lie down, I put on a great coat, and wrapping myself in a quilt, reclined on chairs with my head and shoulders supported on a table. I slept uncomfortably in this position till nearly four, A.M. (Sunday); when I arose my feet and legs were very cold, and my breathing bad. I sat bending forwards at the kitchen fire till eight, A.M., having several "trances" during that time. The morning was very cold, with sleet and snow showers. My difficulty of breathing was greatly increased, when, at nine, A.M., the sitting room was ready for me. I felt no local pain, but a sense of heaviness and tightness in my head and chest; this was not a particularly *spasmodic* attack. Formerly, (previous to the last four or five months,) the spasmodic heavings of the chest, during severe fits, were so violent as to produce a concavity in the abdomen, and to exhibit the muscles of the abdomen like tightly drawn cords, stretching from the chest to the pubis. The chest, on the other hand, was inflated, and much increased in volume, as if the contents of the abdomen were

drawn up, and *crushing* the lungs and heart. These symptoms were not very prominent in the present fit.

There was a profuse flow of colourless, salt-tasted saliva throughout the fit. It stains black cloth a dirty white. There were frequent copious evacuations of limpid urine, and three stools of very offensive smell, voided with difficulty by a succession of spasmodic efforts. Painful exhaustion and increased difficulty of breathing followed each motion. My feet and legs were obstinately cold, and required nearly constant friction, which is grateful to them in a fit. My hands hot, with the veins swollen. Continued ill, restless, and uneasy till about three, A. M. (Monday), when my attendants, after many attempts, succeeded in "laying" me, *i. e.* putting me to rest reclined on chairs on my right side, with my head and shoulders greatly elevated, wrapped in blankets, and near a large fire. I slept till half-past seven, A. M., when I awoke sensible of the departure of the fit, but with considerable difficulty of breathing, weakness, and a distressing sensation in the mouth. After the subsidence of the regular fit, I frequently dread suffocation, owing to extreme difficulty of expectoration, the organs employed in this function being now in a state of torpor produced by their over-excitement during the fit; this was particularly the case to-day. The mucus was tough, and in large portions, and the labour of raising it from the air passages was rendered more exhausting by deficiency of saliva, of which there was a profuse expenditure yesterday.

At half-past nine, A. M., I was able to eat a little toast and tea with half an egg, and at eleven, A. M., to read Knickerbocker. So here ends this fit.

Tuesday, 10th February. Quite well, except weakness

Wednesday, 11th. Slight cold.

Thursday, 12th. Cold increased (by exposure).

Friday, 13th. Pretty well; cold less severe.

Saturday, 14th. Well.

Sunday, 15th. Well.

Monday, 16th. Very well, walking on Newry mountains.

Tuesday, 17th. Very well.

Wednesday, 18th. Well.

Thursday, 19th. Pretty well; cold N. W. wind.

Friday, 20th. Well. "Hippish;" usual interval has elapsed.

Saturday, 21st. Rather well. "Hippish."

Sunday, 22nd. Threatened with asthma. Cold. Slight thaw.

Monday, 23rd. Rather well.

Tuesday, 24th. Fit.

This was a most treacherous attack. I had been threatened on Sunday, the 22nd, but partly as I think from the large use of Irish moss jelly, and partly from the excitement produced by a sudden illness of my sister, (and which excitement was changed to one more pleasant by her rapid recovery). The symptoms disappeared, and I was well on Monday. On Tuesday morning, however, at six o'clock, I was roused from bed by the hateful visit of my persecutor. The symptoms increased in violence till about twelve, A. M., when I was "ill," and remained so till one, A. M. on Wednesday, the 25th, when I was composed to rest on chairs.

This was a mild fit. There was not very much fever, nor much of the horrid taste. No considerable headach nor spasms. The most troublesome circumstance was the very profuse discharge of colourless urine, which continued at short intervals from sunrise till six, P. M. On the day of recovery there is usually a discharge of very high-coloured urine. There is also a distressing feverish languor and nervous irritability caused I suppose by the absorption into the system of those particles which should have been carried off in the urine during the fit.

February, 25th. Recovering.

26th. Well.

27th. Well.

28th. Well.

March 1st. Well.

2nd. Well.

3rd. Well.

4th. Well.

5th. Well.

6th. Pretty well till after dinner.

On this day, immediately after dinner, (at which I ate moderately,) I perceived the rapid approach of a fit. I swallowed a seidlitz powder with a little ginger, followed by a cup of coffee without cream or sugar, and afterwards some spoonsful of moss jelly. These remedies, particularly the immediate exhibition of the seidlitz powder, retarded the approach of the fit, and I think lessened its violence. I was able to read without inconvenience till nearly eight, P. M., when the fit might be said to have "set in." Among the first symptoms were tightness of the chest, or a feeling of the contents of the chest being *crushed* by those of the abdomen; dryness of the nose; coldness of the extremities; wheezing; frequent profuse evacuations of colourless urine. Drowsiness and fulness of the head, with increased coldness of the lower extremities succeeded. I fell asleep at nine P. M., and slept half an hour, or so. I spent the whole night and the next day in the same state, having oppressive drowsiness, without being able to indulge in sleep for more than twenty minutes at a time. Though the wind blew almost a hurricane, so that the exertions of several men were required to keep the thatch on the cottage, I think that instead of rousing it rather lulled me to sleep.

I was not very feverish during this illness. My head was not affected with pain. I used to have a tendency to delirium in the height of the fit; but I think this has not occurred since summer.

Another symptom, which formerly was usual at the approach of a fit, has not appeared for a considerable time. It is difficult to describe—indeed I hope I may never again experience it. At the approach of a great fit, I was subject to lose the consciousness of the circumstances in which I was placed; or from

some observation made by a person present, or from some idea occurring to myself, to *jump* at a conclusion having no connection with the subject, and in utter disregard of the rules of syllogism. Sometimes, both these “ravings,” if I may so call them, would occur frequently in the course of an hour, each being instantly followed by a consciousness of its absurdity. They were invariably accompanied by fulness of the head. I think they generally occurred at the commencement of motion in the open air.

Composed to rest at nine, P. M. (Saturday)

March 8th. Recovering.

9th. Well.

10th. Well.

11th. Well.

12th. Very well.

13th. Very well.

14th. Very well.

} Very fine weather.

15th. Very well till evening.

16th. Fit.

On the evening of Sunday, the 15th, I should have suspected the approach of a fit, on account of inclination to go to bed much earlier than usual, and an unusual feeling of heat over my body. Fit commenced immediately on my rising from bed on Monday morning, at six o'clock, swallowed a dose of epsom salts. A drowsy, uneasy fit, similar to that of the 7th inst.; lasted till six, A. M., Tuesday.

March, 18th. Well.

19th. Well.

20th. Well.

21st. Well.

22nd. Well.

23rd. Very well.

24th. Very well.

25th. Well. In dread of fit, as interval has elapsed.

26th. Ditto.

Ditto.

27th. Well.

28th. Fit commenced.

Immediately on rising this morning, I perceived symptoms of an approaching fit, as weakness of spine, chilliness, dirty mouth. The fit, however, did not extend beyond this state till four or five P. M. At ten, A. M., drank three large cups of strong coffee with very little sugar or cream. At twelve, A. M., I walked very slowly and cautiously to the garden (about 150 yards), where I sat two hours. Having read or heard that aromatics have an antispasmodic tendency, I ate some leaves of lavender and thyme. Fancied that my drowsiness was thereby increased. On returning to the house, I fell asleep for an hour. Awoke before five, P. M., and found the spasmodic action increased. This is always a consequence of sleeping in a fit. It would appear that some of the muscles employed in respiration in the fit, require to be stimulated by the will, which, not being applied during sleep, the lungs become oppressed by the insufficient action of those muscles, which are, as it were, borrowed for the occasional assistance of the overburthened lungs. The most prominent symptoms were extreme weakness of the spine along its whole extent, and spasmodic heaving of the chest.

This was a mild fit ; it was over at about eight, A. M., on Sunday, the 29th.

March 30. Recovering well.

31st. Well.

April 1st. Well.

2nd. Well.

3rd. Pretty well.

4th. Well.

5th. Well.

6th. Well, but spine painful.

7th. Well.

8th. Well.

9th. Well.

10th. Well.

11th. Well.

April 12th. Well.

13th. Well.

14th. Well.

15th. Well.

16th. Well.

17th. Fit. At home.

} Have no doubt that this interval was prolonged by my leaving home. Not at home shortly before the probable access of a fit.

On this morning I rose soon after sunrise with premonitory symptoms. I fell asleep at the fire for a short time, and on awakening, found the symptoms aggravated. Took hippo wine to the extent of producing vomiting. Became painfully drowsy, and closed till evening, when the fit assumed the *uneasy* form or stage, and my spine was extremely weak and painful. Spine never was worse. Did not become settled for rest till four, A.M. Saturday.—Rose at half past seven, A.M.; very weak all day, and spine distressingly ill, so that I was obliged to throw myself on my back for ease.

April 19th.—Recovering, but “not as well as can be expected.”

20th. Difficult expectoration; pretty well.

21st. Pretty well.

22nd. Ditto, but have caught cold.

23rd. Well.

24th. Well.

} Fine weather.

25th. This was a very cold day; the wind blew hard from N. W. (I hate the N. W.) with hail and rain showers. In the intervals between the showers I sowed seeds, &c. in a flower garden, in a sheltered situation. I had no difficulty of breathing, but the abdomen feels sore to the touch. Bowels have been constipated since last fit. On going into the house for the evening, I perceived abundant symptoms of what I supposed a severe cold. [I did not dread a fit of asthma, as my *time would not be up* until nearly this day week.] After hesitating between the choice of castor oil and that of salts, I swallowed (faugh!) a dose of the latter. I became very drowsy and hot,

and after remaining in stupor for two hours, went to bed at half past ten ; awoke about twelve or one, very stupid and feverish, and with oppressed breathing. I sat at the fire with very insufficient covering till morning, having passed the interval in dozes. The night was very severe, and the symptoms were aggravated in the morning. Very ill all day, and till half past four o'clock on Monday morning, 27th, when I was "laid." Awoke at half-past seven with very bad breathing, and a painfully weak spine ; in short, altogether disordered. Was not even tolerably well (I mean able to eat any thing) till past ten, A. M., when I breakfasted on coffee, with scarcely any sugar or cream. Very difficult expectoration all day. Dare not go to bed on account of the obstruction in my chest. This was a very bad fit.

Tuesday, 28th April. Slept badly. My dreams had such evident connexion with the state of my respiratory organs that I shall describe one of them. I fancied myself involved in the confusion of an *abattoir* or public slaughter-house, in the twilight of a hazy evening, and while the butchers were engaged at their horrid office. The air felt thick with the last breaths of animals expiring all around me. Wherever I turned I was met by abominable smells and "sights and shapes unholy." I was afraid to breathe. I could see only so much as to show the difficulty of extricating myself. In attempting to escape I entered dark, narrow passages, which grew darker and narrower and more mephitic as I advanced. I became *wedged* in one of them, and in horrors awoke. On awaking, I immediately and instinctively assumed the erect position, by which my breathing was relieved. All this day my breathing was difficult, and expectoration almost impossible. Could eat nothing, or rather was afraid to eat. Got on horseback, but was unable to bear any motion quicker than walking (of the horse). Was better after the exercise.

Wednesday, 29th April. Thursday, 30th April. Disagreeable dreams both these nights, but I was not so ill as on the 27th. In these dreams I wanted the power of *expiration*.

Breathing oppressed both days, particularly after any exertion of *the trunk*. The labour of dressing was very severe.

On Friday, May 1st, (a sunny day, which I love), about 12 A. M. my breathing became less difficult, and I began to recover in every respect. Did not, however, eat anything stronger than bread and butter.

May 2nd. Pretty well. Head aches.

3rd. Very well.

4th. Very well. Spat thick mucus in the morning.

5th. Well.

6th. Well.

7th. Very well till evening.

8th. Threatened. Roused at half-past 4, A. M. Spine weak. Skin hot. Took camphor.

9th. Poorly. Very nervous. Miserably vexed for a short time by slight causes.

10th. Poorly. Took camphor at bed-time.

11th. Fit commencing.

11th May, (Monday.) I was so foolish yesterday, as to dine on ham and green hearts (early cabbage.) I say "foolish," because I had been warned in the morning, and besides *my time was up*. My breathing being affected at bed-time, I took a dose of camphor mixture. I was roused at five this morning, by an affection of the bowels, which very frequently ushers in the fit. Spine weak; pulse irregular. Took another dose of camphor mixture. My pulse became much quicker, and other effects of its stimulating qualities continued for some hours, when the spasmodic heaving of my chest induced me to apply to strong coffee in repeated doses. Did not become "ill" till 4 P. M. [I never pronounce myself "ill" while I am able to read. I cannot see how or why the accession of the paroxysm can influence my appetite for *reading*; but the fact may be worth mentioning, that at this time I invariably feel myself, as it were, compelled to read, and this with scarcely any selection

of my subject. The book I place at an elevation of a few inches above the chair on which I sit, while I lean on it resting my head on my hand, and one or both elbows on my knees. Even when (as is frequently the case at the commencement) my eyes are inflamed or tender, the disposition to read is as strong as ever. When the stupor is oppressive, I read in the intervals, though only of a few minutes' duration.] At 4 p. m. the drowsiness became overwhelming, and "illness" soon followed. I did not, however, grow "very ill," and at 10 p. m. was composed to rest. The spine was not so much affected as usual. At half-past 3 A.M. (Tuesday, 12th,) I awoke with an extremely acid stomach, and having left my couch for the purpose of washing my mouth, I did not return to it. Read for some time, and then finding myself less sick than usual at the commencement of the recovery, walked at 5 o'clock to the garden, (fine morning.) I do not remember that I was ever able to walk erect so soon after an asthmatic fit. Did not suffer much from exhaustion or feverishness, nor at all from spinal weakness or pain. I took a Seidlitz powder before breakfast, at which I ate very little, and spent the greater part of the day in the open air. Dined on rice and milk, almost immediately after swallowing which, I became very drowsy, and on awaking found my breathing oppressed, my stomach ill, my mouth dirty, with a *gluey* secretion, and my whole system disordered. Similar effects have more than once occurred, from dining on milk or rice milk, either previous to a fit or while recovering from one. The treatment I employed in the present case was to eat ginger, and to drink my tea *without cream*. *I recovered after the third cup.*

From the effects produced in this fit, apparently by camphor and coffee, I have great hopes that if judiciously combined with other substances, and administered at proper crises, they might prove of great service in alleviating the miseries of asthmatic sufferers.

In the present instance, however, their good effects on the fit were nearly counterbalanced by loss of appetite, vitiated

taste, and painful nervousness and debility, which troubled me for the four days succeeding that of my recovery. On one of them, (the 15th,) I was even slightly asthmatic for twelve hours. However, it is likely that the bad state of the weather concurred in producing this sickness.

May 17th. Rather well.

18th. Health, spirits, and digestion, all better.

19th. Well.

20th. Very well.

21st. Very well.

22nd. Very well.

23rd. Very well.

24th. Fit.

} Fine weather.

This fit was induced chiefly by exposure to sudden changes of temperature. I felt so very well on Saturday the 23rd, that I omitted putting on my flannel shirt in the morning, (I generally lay it aside in warm weather, except during a fit). The day turning out showery, however, I replaced it. I rode to Newry (a distance of five miles) in the morning which was temperate. The wind had changed to north in the evening, when I returned. After sun-set, the air being very cold, I went suddenly from an overheated apartment into the open air without muffling, and sauntered about half a mile to a friend's house, whose sitting apartment is kept at an extremely high temperature; and having remained there fifteen or twenty minutes, returned through an atmosphere now colder than before, and seating myself before a fire, nodded awhile, and then went stupidly to bed. Premonitory symptoms roused me the next morning, (24th,) at 3 o'clock. In order to give coffee a fair trial, I (using as slight exertion as possible, the slightest movement of the trunk always exacerbating the symptoms,) made some very strong coffee, and swallowed three large cups of it without sugar or cream at 5, A.M. and another at 10, A.M. There was heavy rain all day. I had my feet and legs immersed in hot water at 10, A.M. and at

3, P. M., each time for at least ten minutes. A friend whom I had not seen for twelve months, having come to visit me, I was induced to remain in the same apartment with him during the evening. Before his arrival I was not "very ill," and the coffee and foot-bath were probably of service, but certain it is that the pleasurable excitement of his conversation (in which I soon began to join) entirely removed the asthmatic fit. The lively interest I felt in my friend and in the subjects of the conversation, co-operating with the action of large doses of very strong coffee on an empty stomach, and a relaxed state of body, produced a very high degree of nervous excitement. I felt, as Sir Humphry Davy describes himself to have felt while breathing nitrous oxide. As my difficulty of breathing disappeared I grew more and more flippant, till at length I was making regular (or rather irregular) speeches. I indulged my declamatory propensity to a rather dangerous extent, as appeared by my falling into a slight swoon shortly after my friend's departure. The matter expectorated that night was tinged with blood. However, I went to rest (*not to bed*) in high glee at my victory over my enemy, asthma.

May 25th. Recovering. Exhausted, "stuffed," and with a dirty mouth in the morning. Exercised and spent much time in the open air. Dug, &c. in my flower garden. Have an odd notion that breathing over fresh mould is salubrious. I bathed in the lake once or twice. Threw off flannel. Lay on my left side five nights, which is very unusual.

May 26th. Well.

27th. Very well.

28th. Very well.

29th. Very well.

30th. Very well.

31st. Well.

June 1st. Pretty well.

2nd. Well.

3rd. Very well.

} Fine weather.

4th. Very well.

5th. Well.

6th and 7th. Threatened.

} Fine weather.

On Saturday, the 6th, when paying a visit I unluckily gained the good graces of an old lady who treated me to custard. On tasting it I perceived that it was rather acid, but rustic politeness prevented me from appearing to dislike it. On my way home I was full of apprehension respecting its evil consequences, which feeling, no doubt, tended to render my stomach more obnoxious to the influence which acids with milk generally exert on it. On reaching home I attempted to read, but found it impossible, on account of the stupor which overwhelmed me. I remained *thick headed*, (I think this term best describes my state,) heavy and dejected, with a very acid mouth, for some hours. Tea, however, revived me. On the next morning, when rising, my mouth was nauseously acid, but the effects of the sour custard disappeared finally after breakfast.

June 8th. Very well.

9th. Very well.

10th. Very well.

11th. Very well.

} Very fine weather. Exercised
in open air. Bathed in lake
each day.

June 12th. Threatened all day ; ate nothing ; sat in the garden and read much ; imprudently crawled (I cannot call it walking) about half a mile to keep an appointment ; took a Seidlitz powder ; spent an uneasy night, though not very ill.

About nine o'clock next morning I was so far recovered as to be able to eat breakfast, and considered myself over the fit. So I was, had I behaved with prudence. Owing to a complication of circumstances, and in order to fulfil an engagement, having drank a glass of wine as a stimulant to my weak and feverish frame, I walked in the heat of the day a distance of more than a mile, and thence drove four miles, and back again. I felt no uneasiness from this more than fatigue, and I spent rather a pleasant evening ; but in the excess of my foolhardiness, I walked

home (one mile) at 10 o'clock in a heavy, cold,* foggy evening. On reaching home I laid myself to rest before a fire, but was soon roused by an attack of asthma, which continued till Monday morning the 15th. (Saturday was the 13th.)

On Monday, June 15th, I was weak and exhausted; weighed myself, and found that I had lost ten or twelve pounds since last Wednesday.

On Tuesday, June 16th, I was fast recovering, and had a fair prospect of being soon quite well, but unfortunately, my appetite being generally sharp on the day after recovery, I ate meat three times in the course of the day; the consequence was, a smart fit, which lasted from 1, A.M. till 12, P.M., of Wednesday the 17th.

On Thursday, June 18th, I felt well, and did not suffer from weakness of the spine nor feverishness, as usual after a fit. The wind (not a high wind) was north-west, but while the sun was up the temperature was agreeable; after sunset, however, it was cold and damp; I incautiously sauntered for nearly an hour in the twilight, feeling cold in my face at the same time.

Friday, June 19th. A dark, damp day; on rising I forced up a considerable quantity of thready mucus; became gradually ill. At 12 o'clock, very ill, with intense dyspnœa; cold legs and *thighs*; hot face; laborious spasms. I had my feet and legs immersed in hot water for nearly ten minutes, then rubbed by the warm hands of attendants, and clad in warm woollen stockings; the dyspnœa and spasms were almost instantly relieved, and I fell into a gentle sleep. I spent the remainder of the evening very tolerably, with only a *tendency* to spasmodic respiration on moving my trunk.

* There is a lake adjoining my residence, and several bogs in the neighbourhood. The soil is heavy clay, and there is rank vegetation.

ART. III.—*On some Effects resulting from Wounds of Nerves.*

By JOHN HAMILTON, L.R.C.S.

WITH the exception of the portio dura of the seventh, the supra and infra orbitar, and inferior dental branch of the fifth pair of nerves, the cerebral nerves are not often the subjects of injury. These injuries have been so well described by Sir Charles Bell, and in the various records of operations for tic douloureux, that any further mention of them would be superfluous. The spinal nerves are, on the other hand, often wounded, and as their function is compound, presiding as they do over sensation and motion, when one of them going to the supply of any part, is completely divided, the result is a loss of feeling, of the power of motion, and a diminution of the nutrient functions, with various morbid sensations in the part.

A woman came to me, who had quite lost the use of the left hand ; the fingers were kept stiff and straight, and though they could be flexed by another person, she had no power over them herself. All the parts supplied by the median nerve, viz. the thumb, first and second fingers, and radial side of the ring finger, were quite insensible and emaciated ; the insensibility to touch was such that she said I might cut her finger off without her feeling it. She sometimes had a tingling feel in them, and darts of pain from the fingers up to the shoulders, and when she coughed, in the reverse direction, from the shoulder to the fingers. Four months ago a deep-seated whitlow followed a cut on her thumb, and several incisions were necessary to let out the matter. The scar of one of these is visible on the front of the wrist, on the ulnar side of the pronator radii tendon, where the median nerve is most superficial, by this incision the nerve doubtless was divided, and the present mischief followed. It offers a wholesome caution, that in making incisions in paronychia, nerves as well as arteries require to be avoided. Mr. Graves has under his care a very similar case : a young woman fell on a jug, which breaking cut her severely ; the worst cut was

on the front of the forearm, near the wrist, crossing exactly the situation of the median nerve, which, as the wound was very deep, it no doubt completely divided. Immediately after she lost all sensation and power of motion in the thumb, fore, and middle fingers, and radial side of ring finger, and palm of the hand as far as the wrist, except on the ulnar side ; the back of the hand was unaffected. The affected parts had a sensation of pins and needles, and were numb.

Mr. Swan divided the sciatic nerve in a rabbit ; at the end of two months it began to have much use of its leg, and at the end of four months had perfectly regained it. On examination, union at the divided part was found complete. From other experiments it appears, that at an earlier period, though the nerve had united, it had not recovered its former state so completely as to have afforded nervous supply enough for the proper discharge of the functions of the part. In the first woman, as four months had elapsed since the injury, and no amendment manifested itself, I looked on recovery as hopeless, and that something had impeded the union of the divided nerve. The other, on the contrary, is a fair specimen of the gradual return to the natural state in proportion as the union becomes effected. Three weeks after the receipt of the injury, sensation was beginning to return in the thumb, palm of the hand, and radial side of the ring finger.

The ulnar and radial nerves enter so largely into the supply of the hand and fingers, that after the division of the median nerve, they still suffice to carry on the vital functions ; the symptoms are, therefore, less striking than where a nerve is divided, on which almost the entire support of a limb depends. With the solitary exception of the internal saphenus nerve, the leg is supplied by the sciatic nerve alone, when this nerve is divided, besides the symptoms already detailed, mortification is very often a consequence. In a case of this sort, where, however, the nerve was not quite divided, the symptoms were so intolerable, pain, insensibility, swelling in the leg on the least exer-

tion, emaciation and contraction of the foot, that the patient submitted to amputation below the knee five years after. Though the operation was of great benefit to him, yet, what is a curious physiological fact, at the end of twenty years, when I saw him, he still occasionally suffered from precisely similar painful affections, as if in the *foot*. Such results of division of nerves as these are sufficiently simple, but there is a peculiar train of symptoms sometimes observed to follow the puncture, partial division, or bruise of a nerve, not only the main nerve, but of any branch, however minute or apparently unimportant, scarcely less dangerous and often infinitely more distressing, which is very obscure, and hard of explanation. Detached cases (particularly from the puncture in venesection) have from time to time been published, but till very lately much confusion existed on the subject. Benjamin Bell,* Camper,† and Abernethy,‡ obviously confounded these symptoms from wounded nerve with those of wounded tendons or fasciæ. There is also a point of view in which they have strong claims to our attention, the great uncertainty and general insufficiency of the usual modes of treatment; some of the highest authorities offering precisely opposite opinions. As it is only by a fair detail of cases, and the effect of the treatment resorted to in them, that just conclusions can be looked for on this head, I am induced to offer my small contribution of the cases that have occurred to myself, or come to my knowledge, in the practice of others.

The following case presents the symptoms in the mildest form.

CASE I.—A young gentleman cut himself deeply on the radial side of the index finger; next day, about the same hour that he cut himself, he felt a sensation of pins and needles going from the cut up the radial side of the forearm, with a slight

* Bell's Surgery, vol. iii. p. 116. Of Wounds or Pricks in the Nerves and Tendons.

† Camper, Anat. Patholog. lib. i. p. 9.

‡ Abernethy, Surg. Works, vol. ii.

degree of swelling, and tenderness to the touch ; this lasted about two hours, and then went off, with the exception of one part on the forearm, where the tingling sensation continued for a few days, with tenderness, and some swelling.

In the next, the symptoms are more fully marked.

CASE II.—Catherine Cantwell, ætat. 20, apparently in good health, but somewhat hysterical, came to me at the South Eastern Dispensary, with a wound in the palm of the hand. While contending with her brother for an apple, she went to give him a slap, when the palm of her hand came on the point of a knife, which entered at the middle of the ball of the thumb at its inner side, and nearly penetrated through. There was little bleeding, and the wound healed in a few days ; but then a train of symptoms arose of a most troublesome character ; the palm and back of the hand became greatly swollen, the swelling of a pale, œdematous character, exquisitely tender, and extremely painful ; the tenderness was greatest about the cicatrix, and exceeded any thing I had seen in other diseases. She got no sleep at night, when the pain was worse. At the same time a red line sprung up on the front of the foramen, at its external side, like that from inflamed lymphatics, but without the hard and knotty swelling. These symptoms were, for a time, diminished by antiphlogistic means, but recurred again and again, and about two months after the wound, the swelling got so red and large, and the parts about the wound assumed so much the appearance of a collection of matter, that I should have thought there was one, but that I had so often seen nearly all the swelling and redness disappear in a night, and after remaining away a few days, would re-appear at night, in the course of an hour or so, and for days assume a periodical character, increased and red towards evening, and during the night, and decreased and pale in the morning. The pain, if possible, became greater, the tenderness extreme, a sensation of pins and needles was felt in the hand and fingers, most so in the fore-finger and thumb, with pain and shooting up the arm, numbness, and

feeling of deadness, and loss of power. The fingers and thumb were somewhat contracted, and could not be extended without great agony, nor could they be completely flexed, so as to hold any body firmly. She suffered also from hysterical symptoms, globus, palpitations, and low spirits. Having tried the usual routine of antiphlogistic remedies, general and local, as also, latterly, bark and other tonics, with nothing more than a very temporary alleviation, I was desirous of putting her on the use of mercury, but previous to doing so, shewed her to Mr. Crampton. He agreed with me as to the symptoms being the result of a wounded nerve, and as to the propriety of administering mercury, suggesting, also, the local application of belladonna, which, however, had been used. Directly salivation was established, the happiest effects were manifest: the great pain and swelling went away, leaving slight tenderness, slight swelling, of a pale colour, on the back of the hand, and an occasional feeling of pins and needles. These were a long time before going off, three months after there being still the slightest possible œdema on the back of the hand, and a trifling degree of tenderness about the cicatrix and palm. She also complained that at night the pins and needles sensation came on, and the hand and arm, always moist, then became covered with large beads of perspiration. After a time, these symptoms only recurred every second or third night; but, though able to follow her usual employment at the end of nine months, it was at least a year before she was quite well.

The third case, which I saw in the dispensary of the Meath Hospital, under the care of Mr. Smyly, by whose permission I insert it, presents a great similarity to the foregoing, except in the important particular, that mercury, so decidedly useful to Cantwell, in this exercised no beneficial influence. Its termination is also worthy of note.

CASE III.—February 2nd, 1837. Anne Murphy, ætat. 17, of a nervous temperament, but healthy appearance; eight weeks since, while cutting bread, let the knife slip, and cut the

septum between the thumb and forefinger of the left hand ; not deeply, nor did the cut bleed much, but it was near a month healing. The wound was unusually painful, after the third day it became particularly so, and since then she has suffered pain in the thumb, forefinger, back of the hand, up the forearm, and inside of the arm to the axilla. Worse at night, with occasional intervals of ease of two or three hours' duration. At first, the pain was attended with swelling and redness on the back of the hand, but at present there is no swelling. The severest pain she now suffers is in the wrist, of a most overcoming character ; the thumb has the sensation of pins and needles ; the cicatrix is not hard, but so tender that very moderate pressure cannot be borne ; the hand sweats at night.

20th February. Is taking calomel and opium ; the pain is worse, and is now on the outside as well as the inside of her arm and forearm, about the shoulder, and up the side of neck ; it is now also worse at night than in the day. As the mercury was found to have no effect it was omitted.

March 4th. Worse, the pain extending down the small of back and about the hips.

May 16th. Since the last report she went into Stevens' Hospital, where, among other remedies, electro-magnetism was tried ; but she thinks, if anything, she was worse after it. The tourniquet was also ineffectually used. To the other symptoms has been added that of œdema, of a pale, slightly mottled colour, from the hand up to the axilla. She says that the œdema is not constant, agreeing in that respect with the previous case, but occasionally becomes considerably lessened, at which periods the pain is more severe. The whole arm is tender to the touch. She has taken all the usual tonics, iron, quinine, &c. narcotics, conium, hyosciamus, &c. been leeches, blistered ; had belladonna and mercurial ointment, lead lotion, &c. applied topically, and is taking arsenic. She has now laboured under the disease five months, and it is if anything worse.

June 14th. Mr. Quinan told me that some days since she got a fright in the street and fell into an hysterical fit, and had to be carried home ; for a couple of days after recovering from it she was affected with a constant sobbing, and when this ceased, hiccup of a most violent kind began, with difficulty of swallowing, and pain and tenderness in the epigastrium, apparently the result of the violent action of the diaphragm. After the fit all pain and swelling left the arm, and have never returned. The sudden violent impression on the nervous symptom generally, having quite mastered the local affection.

By the kindness of Mr. Crampton, I am enabled to present the next case, which offers a still severer form of the affection.

A lady, about eighteen years of age, was bled in the right arm, December 2nd, 1815. No particular sensation was felt at the time. On the evening of the following day and the next morning the arm felt unusually stiff, which was attributed to its having been too much used in writing ; but while driving in a carriage that day she was seized with the most intolerable pain from the part where the lancet entered, down to the finger's ends. She thought it the most acute pain she had ever felt ; it lasted for a minute or two, often returning, accompanied with an odd tingling sensation. After a short time the intervals of ease became longer, and little uneasiness was felt as long as the arm was kept quiet. The next day the pain increased considerably, the arm could not be straightened, was slightly swollen, and the hand felt numb and cold. For a day or two the pain remitted, but afterwards increased, till it became almost insupportable, and extended to the side of the face, producing dreadful suffering. The stomach became constantly sick ; there was total disinclination to food, and in spite of opium the nights were restless. Change of weather had a marked effect on the pain, which became always much worse during frost. This state continued little affected by any remedies till the following March, when the intervals of ease became longer. On the 3rd of April, 1816, she got a slight blow on the back of the hand ;

little was felt at the time, but shortly after the spot became swelled and inflamed, and she became subject to occasional attacks of inflammation of the wrist, accompanied by pain “past description.” Leeches invariably afforded relief. From this time till July, she suffered much from pain in the arm and these attacks; her health was greatly injured. She was nearly entirely confined to her bed, and her spirits were greatly dejected. At this time a new and most distressing symptom was added: the thumb and fingers became permanently contracted to the greatest degree; the least attempt to straighten them was attended with excruciating pain. The pain in the arm, with few variations, was most severe and constant, felt equally night and day, confined from the spot where the lancet entered to the extremities of the fingers, but most severe in the arms. She was quite incapable of any exertion. Extreme pressure afforded her more relief for the time than anything else, but on its removal it was followed by an increase of pain.

Sir Astley Cooper, John Bell, Dr. Baillie, Mr. H. Cline, were all consulted on this case, chiefly with regard to the propriety of excising a portion of the wounded nerve, as a means of removing the distressing symptoms. In a letter to Sir Astley Cooper, Mr. Crampton says, “this young lady’s general health is now much improved, and the nervous irritation seems now to be confined to the injured nerve, and to the parts in immediate contact with it. The pain is still most severe, and extends from the seat of the puncture to the extremities of the fingers; but the most distressing symptom is a violent spasmodic contraction of the flexor muscles, in consequence of which, the thumb and the fingers are (when not maintained in an extended position by means of a mechanical contrivance) clenched with so much force as to overcome almost any power which can be employed to resist it. This inordinate action of the flexors has no tendency to diminish their powers, but the contrary. The muscles, by this contest, seem to acquire an increase of strength every day, as it now requires nearly the whole strength of both my hands to ex-

tend a finger that had escaped from its confinement. Under all these circumstances, I feel strongly disposed to divide the irritated nerve, or rather to cut out that portion of it, on which the wound was inflicted. The bleeding was performed in the cephalic vein, and the high degree of probability is, that the point of the lancet was pushed through the vein into the trunk of the musculo-cutaneous nerve, which, at the bend of the arm, nineteen times out of twenty, accompanies the cephalic vein, and lies directly beneath it. I imagine I shall have no difficulty in finding the nerve just as it emerges from beneath the biceps muscle to pass over to the vein. There is a case reported by Mr. Watson in the second volume of the *Medical Communications*, p. 251, which is strongly in point. I cannot, however, help thinking, that the favourable issue of this case is to be attributed to the division of the nerve, rather than of the fascia. You may, perhaps, too, recollect a case related by Mr. John Sherwin, in the fourth volume of *Duncan's Medical Commentaries*. These are the only well attested instances that I can find on record, in which an operation has been performed with success, for the relief of the symptoms which have succeeded to the puncture of a nerve in phlebotomy." Dr. Baillie and Sir Astley Cooper alone acceded to the propriety of the operation. The latter, however, suggested the previous trial of the electric aura. This produced no effect, and matters daily becoming worse, Mr. Crampton performed the operation, December, 1816. At this time the pain and spasmodic contraction were worse than ever, and the latter had increased to such a pitch, that in spite of an instrument, which, by means of a screw, tended to open the hand, the nails had become buried in the flesh of the palm, giving rise to a foul ulcer ; and what is also not a little singular, the arm had become covered with hair.

The nerve was easily found, as it runs along the outer ridge of the tendon of the biceps muscle, and about a quarter of an inch of it was removed. This part of the operation was performed with the utmost difficulty, in consequence of the con-

tracted position of the arm, and the violent spasmodic motions which were induced by the irritation of the operation. The immediate effect of the division of the nerve was the complete relaxation of the fingers, which had been contracted many months, and freedom from pain. In about twenty hours, however, the spasmodic motions recurred with the greatest violence, and every part of the body was agitated by the most frightful spasms for several days, almost without a moment's intermission. Consciousness, however, was never for a moment suspended, and the pain was inconsiderable. In two or three weeks all tendency to spasm subsided; the general health improved, so that the young lady was able to go into company, and even to dance in the course of the spring of 1817. She still, however, frequently complained of considerable pain in the arm, but the result of the operation was, that she was completely free from attacks of inflammation, or of spasm. In the summer of 1817, a small projecting portion of the cicatrix became exquisitely sensible. The slightest touch on this part produced swooning. In August this irritable part was removed by excision. The operation was succeeded by violent spasms, which lasted for several days, but the wound healed kindly, and has never since been *painful to the touch*. In September the surface of the abdomen became painful upon pressure, and the bowels highly irritable; these symptoms yielded to local blood letting and opiates. Since that period she has suffered two or three attacks of the same kind which have yielded to the same remedies.

In May, 1818, she received a smart blow on the back of the hand; as on a former occasion, this injury was succeeded by inflammation of the wrist. "For the next three months the inflammation recurred with the utmost regularity once a fortnight, then more frequently and at irregular intervals, but since December last with the strictest regularity once a week, at about 6 o'clock in the evening. The pain begins at the seat of the original wound of the arm, shoots upwards to the side of the head, and downwards to the wrists and fingers. The wrist

swells in a few minutes to an extraordinary size, and becomes of a bright and shining red colour: leeches are immediately applied, which in about an hour relieve the excessive pain of the wrist, but a pain remains in the head, which obliges her to keep her bed during the whole of the following day. For the last few weeks, each successive attack has increased in severity and duration. The head is scarcely ever free from some pain, which is rendered intolerable by motion. She is losing flesh rapidly, her appetite has failed, and she has profuse perspirations at night."

At this time she caught cold and became affected with pneumonia, for which she was bled, and put under the influence of mercury: directly the gums were touched, the violence of the nervous paroxysms ceased; from being weekly, a fortnight elapsed before a return, which was milder; then a month intervened, and finally, at the end of between three and four months, she became perfectly free from the complaint. In this instance, the violence of the disease was first checked by the removal of a portion of the wounded nerve, and the cure completed by the accidental exhibition of mercury.

In the course of the correspondence about this case, Mr. Cline mentioned to the father of this lady, the case of the daughter of a surgeon of great eminence in London, "who cut her finger, the wound healed, but soon after she felt an affection of the nerves first in the injured part, then gradually up the arm, and at last a contraction of the whole arm came on, which doubled it up so as to bring her hand upon her shoulder. On trying to bring it back the pain was excruciating. They therefore left it in that position, attending solely to her general health; all other expedients having failed, such as electricity &c. they took her to the sea side, and otherwise took care of her, till all came right in about I believe two years.

Dr. Baillie mentioned a case where a nerve was wounded from strong pressure, and where a painful sensation was communicated from the injured part to the brain, and epileptic fits

were produced. In this instance the injured portion of nerve was cut out. The epileptic fits, however, still continued, and in one of them the person died. On the other hand, Sir Astley Cooper alludes to the case of a man in Guy's Hospital, who received a blow on the thumb, pain soon afterwards began at the part, and extended to the forearm, afterwards it proceeded to his shoulder and neck, and ultimately to his brain. When these fits of pain occurred, he lost his voluntary power, and fell. As this state of the brain was always preceded by pain in the thumb, and by a curious contraction of the arm, Sir Astley Cooper laid bare the radial nerve at the tendon of the supinator radii longus, and removed $\frac{5}{8}$ of an inch of the nerve. The wound quickly healed. The man had a few fits after, but it was thought of a less violent description, and after his return home perfectly recovered. Similar to the foregoing, was a case which I remember to have seen in the Meath Hospital five years since, under the late excellent surgeon Mr. McNamara. The woman was upwards of fifty, and five months previous to admission, had cut the top of her thumb very slightly, but though she lost little blood, the pain was so great, that she nearly fainted. The cut healed quickly, and she remained well two months, when, while wringing clothes, she was suddenly seized with most intense pain in the back of the lower joint of the thumb, which lasted two hours, and was attended with swelling. A slight degree of tenderness and swelling remained, but pain followed the least motion of the thumb, and on striking against any thing, or any unusual exertion, or sometimes spontaneously, severe pain shot up the front of the forearm, inside of the arm, side of neck, and into the head. The arm would become powerless, and a sick, faint feel, and thirst ensued, with great depression of spirits, and often headach. A degree more, and this woman would have epileptic fits as in the preceding cases. There was a diffused, hard, tender swelling round the carpal joint of the thumb. The thumb itself felt numb and cold; the palm of the hand, front of forearm, and inside of arm, very

tender to the touch. She left hospital soon after admission of her own accord.

From the cases here given, and many others that I do not wish to tire my readers with, I feel myself justified in stating, that a wound of a nerve, or any of its branches in peculiar states of the constitution, may be followed by most distressing local and constitutional symptoms, viz. :

1. Pain of the severest character in the injured part, or shooting from it in various directions in the course of the nerves to the extremities, or to the brain. That it is often periodical, and generally attended with the most exquisite tenderness.

2. That with the pain there is frequently redness and swelling, resembling a good deal the appearance of the skin in inflammation of the fascia or a deep collection of matter, but differing, in being of a paler colour and more œdematous character, in being subject to sudden increase and diminution, and at times being only periodical.

3. Contraction of the limb, and spasms of an unusually violent character; the spasms in some instances going into the genuine convulsion of epilepsy.

4. That the constitutional symptoms are those commonly called nervous and hysterical, viz. : depression of spirits, prostration of strength, globus, &c., but that hectic fever has also presented itself.

5. That all the symptoms uninfluenced by every remedy made use of, have gradually declined after a long interval, having apparently undergone a natural cure. But that in a few instances the termination has been fatal.

It is not easy to account for these symptoms being present in only a few cases of wounded nerve, for in the greater number of injuries daily occurring, in which the nerves must be wounded, they do not ensue. They are most frequently observed in nervous and hysterical women, but not always; and it is on account of their occurring in such subjects that Mr. Brodie has founded the opinion that they depend on hysteria.

This, in common with most of the opinions of this excellent surgeon, deserves every consideration. It cannot, however, escape the attention of any one carefully reading the cases given in this paper, that though there certainly is a very great resemblance to hysteria, yet there is still a most striking difference ; the pain, the spasms, contraction, and peculiar local inflammation, are not chance symptoms, but occur with considerable regularity, and when bad effects follow the wound of a nerve are the symptoms which present themselves with an almost constant uniformity. Now, is this group of symptoms observed, without a wound, in hysteria ? If it ever does present itself, it must be rare indeed.

I rather incline to the opinion, that a peculiar irritable inflammation* is set up in the wounded nerve, and its branches conveying morbid impressions to the brain and spinal marrow, and from thence re-acting on the nervous system generally.

This was pretty much the opinion held by Mr. Abernethy. Mr. Brodie's opinion, that the symptoms arise from a general hysterical condition, is of the more importance, as it has led him to be opposed to operations on the affected nerve, with a view to the removal of the symptoms. Although fully impressed with his authority, I must confess, that from my own limited experience, but still more from a consideration of the many detached cases published by others, I have been led to come to a very different conclusion. In the case mentioned by Sir Astley Cooper, and also in that of Mr. Crampton, the excision of a portion of the nerve above the wound was followed by relief. Mr. Sherwin's case is also conclusive on this head. The bleeding of a servant maid was followed by pain and tingling in the wound, shooting down to the fingers and up to the deltoid muscle and side of the neck and face ; with spasms and contraction of the hand and arm, and even of the side of the face, so as to resemble locked jaw. There was also an

* Upon examining the divided portion of the nerve the neurilema appeared highly inflamed.—Mr. Tecvan's Case, *Lancet*, vol. xxiii. p. 653.

erysipelatous redness and swelling about the wound and arm, and a red, painful, and tender spot on the pectoral muscle ; she was delirious. After the symptoms had continued a fortnight, a deep incision above the cicatrix quite cured her. Dr. Watson's case was very similar ; venesection, followed by all the preceding symptoms, with the additional interesting fact, of a red swelling over the pectoral muscle, so resembling a collection of matter that a deep incision was made into it, but no pus found. Four times he made an incision above the cicatrix where the bleeding had been performed, and each time with relaxation of the contraction and relief to the symptoms, but till the last time they recurred as soon as the incision healed, probably from re-union of the divided nerve. The last incision he carried down to the bone with permanent relief. Dr. Wilson* of Grantham, in a case where strong convulsions and coma followed a wound of a nerve, divided the nerve *two days* after, and immediate ease and permanent relief followed. In the *Lancet*, a most interesting case is given by Mr. Teevan. The patient, after a punctured wound of the thumb, suffered from the symptoms already described ; pain, spasm, and temporary locked-jaw, contracted state of thumb, &c. After they had continued for two months with great prejudice to the general health, Mr. Brodie was requested to operate, but refused to do so, and gave, among other reasons, the following : " Because he considered that there was probably more than one nerve implicated in the disease, and that if so, the division of the radial nerve would not be attended with success." Mr. Earle differed from Mr. Brodie, and cut about half an inch of the radial nerve at its inferior third. The operation was most painful, succeeded by great nervous irritation and temporary increase of the symptoms. But decided amendment soon took place ; at the end of three weeks his general health was much improved, and he was capable of extending the fingers and thumb without pain, but it was several months before he was completely

* Swan on the Nerves, p. 117.

well. My friend Dr. Watson, of New York, has informed me that Dr. Greene, of that city, related the following case to the Medical Society there. A young girl came to him with a severe neuralgic pain of the face that had existed four months. About two weeks before the pain began to trouble her, she had had the second molar tooth of the lower jaw of the left side extracted, and the pain appeared seated immediately in the place from which the tooth had been drawn. The pain was at first intermitting, coming on every morning at nine o'clock, gradually getting more severe, till at length the patient was thrown into spasms. The paroxysms became more and more frequent and severe, and for the last two months they occurred several times daily, but not at night, the length of the paroxysms varying from five to twenty minutes, her general health suffered, and when she arrived in town, she was much reduced in flesh, with an expression haggard from suffering. On examining the seat of the pain, Dr. G. found the mucous membrane, on the inside of the cheek, adhering high up to the top of the gum, and a small tumour filling the space previously occupied by the tooth. Pressure on the tumour gave no pain; Dr. G. removed it with the scalpel, and afterwards applied caustic to the wound. The tumour was not larger than half a pea, of a very firm, fibrous texture. At the time of the operation, the patient had just recovered from a paroxysm of twenty minutes' duration, but a week after, when the case was read, she had had no return. When a nerve has been cut or wounded, little bulbs or tumours occasionally form on it. Mr. Swan describes a tumour of this sort in the incipient state, after a laceration of the sciatic nerve, from a fragment of broken bone. Portal also says, that after wounds of nerves, hard concretions form on them, which, by partial pressure, produce pain and convulsions, by greater complete paralysis or stupor. In the preceding case, the dental branch of the fifth was most probably wounded in the extraction of the tooth, torn, perhaps by a piece of broken jaw, an instance of which I have seen, and the tumour consequent upon it, by its pressure, gave rise to the symptoms. The next case, although not oc-

curring in man, certainly bears on this question. Mr. Crampton told me that a most valuable horse, while clearing a hedge, suddenly fell lame, and continued so for two years, unable to put his foot under him. Mr. C., on examining the leg, thought he felt a tumour in the situation of the radial nerve. He cut down on it, and found a tumour on the nerve, which he removed, along with a portion of the nerve. On dividing the tumour, he discovered a thorn in its centre. The horse perfectly recovered, so as afterwards to sell for its full value. It would, therefore, appear, that in a great many instances of the most distressing effects from injured nerve, the removal of a portion of the nerve, at or above the seat of the injury, has been attended with the happiest results ; the instances where it has failed are, as far as I am aware, comparatively very rare. If the operation is allowed to be desirable, the earlier it is performed after the appearance of the symptoms, the better. Much suffering is thus prevented, as well as the affections being allowed to become fixed on the general nervous system ; it is on this last account that where the operation is performed late, the recovery is so slow—contrast Dr. Wilson's case with Mr. Teevan's. It may be advanced as a reason against any operation in these cases, that they often gradually get well of themselves. This is quite true ; I have mentioned cases of this sort. But it is generally so prolonged, and the amount of suffering, in the mean time, so enormous—witness Mr. Crampton's case—that few would be disposed for such a termination. My friend Dr. Watson, of New York, had a woman under his care, who cut the sole of her foot with a piece of glass, and wounded a nerve ; she suffered for upwards of six years from severe pain in the foot, and up to the thigh, lameness, and swelling of the foot and leg ; her friends having dissuaded her from any operation. With regard to the question of amputation as a means of cure, I have not sufficient data on which to found an opinion. As far as other means are concerned, the only remedy which seems to have exerted any striking influence, is mercury. Two instances are given in this paper ; one, also, Mr. Sinyly's case, where it failed. I have seen a severe nervous affection

of the wrist, the result, apparently,* of a rupture of a nerve during a violent effort, relieved by the following means. The patient was under my care during Mr. Crampton's absence in England; he was a master butcher, and four years previously, sprained his wrist in killing an ox; soon after he became affected with pain in the wrist, coming on at night, and keeping him awake in the greatest agony for several hours, going off towards morning. The wrist, during the paroxysm, became pale red, and slightly swollen, perspired, and got so hot that he said it smoked. He used to have at his bed-side a large pitcher of very cold water, into which he plunged his hand, and kept it there with the greatest relief till the water became warm, when the pain returned. He had suffered constantly more or less violently for four years, and during that time had applied to nearly every man of any eminence in Dublin; and of course had been subjected to every variety of treatment that skill or ingenuity could suggest, but with scarcely any benefit, as, when he came under Mr. Crampton, he occasionally suffered as severely as at first. There was no appearance of disease in the wrist joint, but though he could use it for slight ordinary purposes, it was too weak for the usual violent efforts of his calling. After trying various things, Mr. Crampton recommended the part to be kept constantly wet with a strong solution of the acetate of lead; it at once afforded marked relief, and at the end of ten days he was just well, and had had no return when I saw him several months after.

ART. IV.—*The Medical Topography of Cove.* By DAVID H. SCOTT, M.D., M. R. C. S., &c. &c.

THE term medical topography appears to me well suited to this paper. Under it may be ranged, either in a limited or

* See Lancet, vol. viii. p. 254, where a case is given of most severe nervous symptoms, from the violent effort of wringing clothes.

an extended manner, according as opportunities for observation permit, several subjects. To enter into all the objects which it properly embraces, is not my intention, neither is it necessary to my present purpose. I shall, therefore, first consider the situation of Cove, or rather that body of land upon part of which the town is built. In an ovoid basin, above seven miles by five, formed by the main land, stretching its promontories forward, and then approaching, and in nearly the most southerly part of Ireland, is situated the Great Island or the Island of Cove. Its extreme length, which is from East to West, is seven miles, and greatest breadth, from North to South, three and a half miles. It is separated on the North from the land, by a narrow channel, and has on the South spread before it a capacious harbour, whose Atlantic waters ebb and flow through a southern outlet between the headlands. In the centre of this outlet, which is nearly one and a half miles in its broadest part, is placed a small island, Spike, which apparently filling up the regular chain of land, gives, from some points of view, to the body of waters, the appearance of an inland lake. At the North West part of the basin are admitted the waters of the River Lee, and at the North East, those of a comparatively small channel; the former flow round the western end of the island, and meet those of the harbour, the latter contribute to the channel which bounds its eastern end. In latitude $51^{\circ}.50'$ North, and longitude $80^{\circ}.18'$ West, it has an extent of 80 or 100 miles of high, and in part mountainous land, separating it at its western side from the Atlantic, while it is removed only a few miles at the opposite and south eastern quarters, from the St. George's Channel, by the arm of land already mentioned,—of course the extent of interposed land diminishes in a S. W. direction, while the South has least of all. An island so circumstanced will partake, in its general outline, of the character which distinguishes the surrounding country,—boldness of elevation, with intersecting valley. It is formed of two hills running parallel from East to West; the northern ridge forms the chief

body of the island, and occupies the whole length, in one-half its course unfolding at either side a considerable tract of slowly inclined ground, and in the other swelling out into a body of table land with rather a bold shore. The southern hill runs from the West to about one-half the extent of the former, and terminates at a small bay, where the first named hill is now seen to take up, and continue the southern line. The surface of this ground is generally even, the undulations are few and moderate, and the depressions no where so deep as to form any remarkable interruption to the continued line. The height of these hills over low water mark may be stated, and chiefly the southern, at between 200 and 300 feet. With such a configuration of land, a number of streams will be supposed to exist; these are not so numerous as at first view might have been expected from the particular disposition of the strata beneath; the chief, and I would say the only one worthy of remark, as it continues to run during the entire year, occupies the centre of the valley formed by the before named hills, and empties itself into the harbour at the eastern termination of the southern hill. At this outlet there is a considerable separation of the opposite hills, and an extent of low ground over which the sea water used formerly to flow. From what has been observed, the soil of this island will be of different description: that on the higher parts of the land generally light and dry, composed of unequal and varying proportions of clay, sand, gravel, pebbles, or shingle, some or all combined, and forming a surface of varying depth; more or less of vegetable matter is in different situations intermixed, and in many parts to such an extent, as to give it the character of rich loam. Much alluvial deposit will of course be found in the low grounds. In some parts, both on the hills and in the valley, are found argillaceous deposits, which preventing the permeation of water gives to those places the character of bogginess. This evil does not now exist in any notable way, since the attention of intelligent agriculturists has been directed to its remedy. I may remark, that carbonate of iron in very

small proportion pervades these soils. Here, as well as in many other places, one is struck with the number of erratic blocks lying beneath or on the surface. In every situation I have observed them, from the highest elevation to the lowest level; they are of quartzose rock, in blunted masses, some weighing upwards of one ton. How they reached their present position, and whence they travelled, we will not, we cannot say. The variety of opinions respecting them shows how wrapt in difficulty their history is. The traditional answer given by the islanders to the querist is very forcible, and may convey some truth, "The deluge has left them here."

The geological character of this island will have some relation to the subject of this paper; I shall, accordingly, make a few remarks upon it. In the South of Ireland the grauwacke rock abounds, forming a great chain of high and undulating land, and along the coast presenting its bold and precipitous barriers to the ocean's progress. The intervening valleys, which are of greater or less depth, according to the degree of elevation of their lateral ridges, are chiefly filled up with limestone of the same period, running parallel to considerable distances, and in many parts expanding into wide beds, and starting up into considerable eminences. The grauwacke formation, however, is that which gives the great characteristic outline. From what has been already said of the position of Cove Island its geological features will connect it with the main land at either extremity; hence it is an interrupted continuation of grauwacke rock from West to East, with a small point of limestone on the south-west shoulder, and a considerable strip of the same stone on the north-west side. After quitting the point above named, the limestone appears to dip beneath the harbour, run east, swelling up a little southward into the fine island Haulboline; then appearing on the northern side of Spike, and no longer observed until intersecting the south-eastern boundary of the harbour. The strata composing the great mass run West to East, forming indentations along the

southern line of the island, are raised to a considerable angle with the horizon, having their dip, as geologists term it, chiefly south ; in colour, and degree of cohesion, they differ in several parts of the island ; all make good building stone, and have been used in raising the houses of the town. The fossils which have been here found in this transition rock, are only one or two species of “ *Terebratula*.” Others, no doubt, exist which are so numerously disposed in the same rock in this part of the country.

As the wells or springs of a district have a connexion as percolating through, retained by, or acting upon the rocky strata, or the surface which covers them, they become next most interesting objects of attention. Pure springs in the neighbourhood of a large population are an inestimable blessing. From the position of the layers of rock that has been just now noticed, rains will descend very quickly through them until arrested by some impermeable strata. Such springs as supply water during the entire year are of an uniform temperature ; those which cease to flow during some of the summer months, and whose supply depends more upon the present fall of rain than internal resources, vary their temperature. The former range between 51° and 52° . In last December, when the thermometer stood at 30° on the high ground beside those springs, they indicated a temperature of 51° , and in July, the relation was 76° to 52° . These waters have a source altogether distant from external supply ; in the same quantity and with the same power they have uniformly flowed in the driest years. Their temperature is alike uninfluenced by atmospherical variations. If we suppose, according to Professor Kupffer, that springs rise with an uniform temperature throughout the year, beneath a depth of from eighty to ninety feet from the surface, their ascent must be quick, since they experience only one degree of variation of heat from winter to summer. This temperature exactly agrees with the mean of our climate, as we shall see in another place. The saline impregnations are traces of sulphate of lime and carbonate of lime

derived from the rocks through which they pass; in none of them have I been able to detect any form of iron, though, as already observed, it is found in the soil in several places, and is here and there observed in the form of pyrites, though in small quantity, in some parts of the island.

Observing the order which has been adopted, a notice of the organized living bodies should here follow. Those existences deriving their support in part from the island, and of which it forms the habitat, would appear to claim our attention; but when we consider the vast influence which the atmosphere exercises over them, and the dependence in which both animal and vegetable life are placed towards it, we may fairly be allowed to give a prior attention to those important phenomena which so largely constitute climate. I shall therefore enter rather fully, as its importance demands, into this part of the subject, and as the object of this paper is to endeavour to shew the distinguishing characters of the climate of Cove, of which much has been spoken, I hope to be able to introduce such a detail of observations as will place it in a proper view. It is necessary that I should state how the town of Cove is situated.

The chain of high land which forms partly the South of this island, has been before noticed; on the southern acclivity of that hill, and running West and East, but inclined rather to the west of south in its aspect, is built the town: along this slope it extends a mile; and consists of terraces, ascending upwards and backwards from within a few feet of elevation over water mark, to a considerable height up this rising ground. These lines of houses face the south, but are in some parts modified into a description of street; there is also a square. The elevation is from 13° to 15° , but there is one part at the west end, where there is a depression of land between the opposite high ground on the southern part, giving a slight interruption to the fulness of the outline, and which forms a lesser angle with the level of the water below, than that now mentioned. The site on which the town stands describes a section of a large circle, whose

concavity looks southward, with rather a westerly inclination, and which from this configuration enjoys, as will be seen by the influence of the particular winds upon it, a degree of protection is enhanced by the cheering visits of every beam of sunshine during our colder seasons. From the different degrees of elevation which are here observed, there must be of necessity a variety in its temperature and other atmospheric properties, both in winter and in summer. The observations relate, therefore, almost exclusively, except where for the purpose of comparison they shall be noted in more elevated situations, to the lower part of the town, whose climate they give for a regular and uninterrupted term of three years, commencing in September of 1834, and ending August, 1837. During this period the instruments employed were: Sykes's self-registering day and night thermometers, suspended in a northern aspect, and removed completely from all direct influence of the sun; a similar registering thermometer, with a blackened bulb, and exposed to the direct influence of the solar beams, for a term of one year, viz., September, 1834, to August, 1835, inclusive; Daniel's hygrometer, and a Fahrenheit thermometer; a pluviometer and barometer, at about seventy feet over the water's level. The registering thermometers were noted daily, with the general state of weather; the hygrometer three times daily, with the direction and force of wind and appearance of weather at each observation; the barometer likewise three times in the day, and at the same hours as the hygrometer, viz., 9 A. M., 3 P. M., and 9 P. M.; the pluviometer every morning according to the necessity, at 9 A. M. Before entering into any particulars, I think it will be better to give first a general idea of our climate, deduced from the mean observations of the three years, followed by their distribution in the respective months.

The mean annual temperature, which is made up of the mean maximum 56.7° , and mean minimum 46.5° , amounts to 51.6° . The mean dew point may be stated at 46° , which is a

small point of a decimal below the means of the hours of observation, and which supposes that a night observation is included; the mean minimum temperature falls to 46.5° , and the mean of the dew point sinks only 0.5 below this; from the morning to the afternoon it rises 0.6 of a degree, from afternoon to night it falls 0.3; and beyond this again, supposing a midnight observation to have been taken, it falls 0.2. The temperature of the vapour thus, when all influence is withdrawn, is nearly that of the air during night. The degree of dryness for this term is 5.6° expressed on the thermometer, and the amount of moisture, calculated on the hygrometric scale, is .829. The mean weight of the aqueous vapour suspended in a cubic foot of air, is 3.662 grains. The greatest degree of dryness is 24° ; and least observed degree of moisture .436. The mean weight of the atmosphere 29.996 inches. The barometer has not observed a regular descent from morning to afternoon. On looking over the several columns, I find that the mercury stood higher at 3 P.M. than at 9 A.M., during ten months of the whole period, yet the sum of each hour stands:—

9 A. M.	3 P. M.	9 P. M.
29.991	29.984	30.016

which gives a fall from morning to afternoon of .007 inch, and a rise from afternoon to evening of 0.32 inch. Its extreme range is 1.94 inches; from absolute maximum 30.84, to absolute minimum pressure 28.90 inches. The fall of rain is 33.299 inches, and the evaporation, which is calculated from the mean temperature and dew point, averages 25.643 inches. The duration of each wind during this time was from the South, 96 days; South-west, 222 days; West, 159; North-west, 213 days; North, 132; North-east, 84; East 78; and South-east, 111 days.

Having thus given a general idea of the means of the year, I shall now proceed to those of the months, and shall first commence with January. This month is the coldest in the year, and is made up of a mean maximum temperature, 46.6° ,

and a mean minimum, 40.4° , constituting a mean of 43.5° . The mean temperature of the moisture, or the dew point, is likewise less, with the exception of March, than that of the other months, being 40.3, and as nearly as possible corresponding to the mean of the maximum temperature of the month. The mean degree of dryness is 3.2° , and the mean degree of moisture expressed on the hygrometric scale is .894, while the least degree of moisture is .707; the mean weight of moisture in a cubic foot of atmospheric air 2.982 grains; the mean barometrical height deduced from three columns 30.034 inches; its mean range 1.51 inches. The average influence of the sun's rays is 4.7° , and its greatest power 9° ; the mean direct heat communicated to the blackened ball amounting to 52.6° . Though the sun's radiant heat was noted but for one year, and though I am aware of the disadvantages attending both the position chosen, and the means employed to concentrate the rays, yet an approximation to its influence on our town may be obtained, and perhaps not far from the actual intensity. The fall of rain 3.578 inches, and the evaporation calculated from the hygrometer .961 inches. This mode of calculating the evaporation appears to approach nearer to the actual quantity taken up into the air, than the result obtained from the ordinary covered vessel, which, from the very means taken to prevent the indropping of descending rain, must necessarily present material obstruction to the free elevation of vapour: other objections may also be made against its employment. From a comparison of the result in both methods, I have been disposed to adopt the mode above named, after the rule given by Professor Daniell in his admirable *Essay on the Hygrometer*. According to the amended tables accompanying that instrument, it is necessary to state, the hygrometrical calculations have been made. The south-west wind exceeds those from the other points, its mean duration being $8\frac{2}{3}$ days.

In February we have an increase of over one degree of temperature, but obtained entirely during the day; the mean tempe-

rature is 44.4° , the mean minimum being 40.5° , and the mean maximum 48.4° , and the range 7.9° . This want of progress in the minimum will arise from the less clouded and comparatively clearer sky permitting terrestrial radiation to go on more freely during the night. The advance made in the dew point is less than we would be inclined to conjecture from even the small increase in heat, the mean being 40.4° ; this retardation will arise from the description of winds now blowing, and which, in another place, will be considered. The monthly range is 17.3° ; the mean degree of dryness increases to 4.0° ; the degree of moisture to .862 diminishes; and the least degree of saturation is .678. The mean contents of vapour in a cubic foot of air is 2.977 grains. The force of solar radiation is 8.2° , while its greatest power is 19° , the mean having increased to 57.2. Mean pressure 29.940, and mean range 1.40 inches. The amount of precipitation 4.048 inches, and of evaporation 1.76 inches. The North West wind makes up a total of nine days, exceeding in prevalence each of the others. The mean temperature of March is 45.3° , an increase acquired while the sun is above the horizon; the mean minimum not advancing beyond 39.7 while the mean maximum is 50.8. This low degree of nightly temperature, as well as that of one or two other months, has been produced, in the average, by the severe winter and spring just past, and whose rigors were relatively felt here; the mean minimum temperature of the same month, in 1836, was 40.1° , while that of March 1837, was reduced to 36.9° . The same causes which tended to keep down the temperature of the air, will also have affected the distribution of vapour; the mean dew point is only 38.2° , which difference in amount with the two former years is from 5 to 6° . The mean degree of dryness is 7.1° . The degree of saturation is reduced to .775, and the mean of the least point of saturation to .589. The mean weight of vapour in a cubic foot of air, 2.767 grains. The sun's mean heat increases to 13.7° , its highest influence being 26° , and the absolute mean 66.9° . The mean height of barometer 29.909

inches, range 1.41 inches. The mean deposition is 1.931 inches, whilst the sum evaporated increases to 2.201 inches. The prevailing wind is North West, blowing, in the average, eight days.

We have an advance of 3° in the mean temperature of April, chiefly acquired during the day, the mean minimum 42.0° , the mean maximum 54.3° , medium 48.3° . The solar radiation amounts now to 22.3° , its greatest force 42° . The mean dew point is 40.7° , advancing with the increase of temperature, but yet relatively kept down by the operation of distant causes. The mean dryness is 7.5° ; the mean degree of saturation .749; mean of the least degree of saturation .515. A cubic foot of the atmospheric air contains 2.952 grains of moisture; the mean height of the mercurial column 30.069 inches, having a range of 1.22 inches. Characterized rather by showers than falls of rain, the mean amount of deposition is small, not exceeding 1.294 inches; the sum evaporated 2.640 inches. The prevailing wind of the last is likewise that of this month, the number of times that the North West wind blows making $7\frac{1}{3}$ days.

In May, the mean temperature of the months is advancing fast, as here it reaches 54.7° . The days and nights increase in nearly the same proportion, the mean maximum is 62.1° while the mean minimum is 47.2° . The mean dew point is 45.9° . Now is the temperature of the air markedly exceeding that of its aqueous contents, and the degree of dryness consequently raised. It is 8.8° , and has arrived at its maximum. The degree of saturation amounts to the smallest proportion, this month being .736, and likewise the mean of the least degree of saturation decreases to its minimum .489. The weight of moisture in a cubic foot is 3.509 grains. The barometer stands also higher than in any other month, its mean indication being 30.106 inches, and its range is the least 0.81 inches. The sun's influence is 31.6° , the greatest power 36° . The absolute mean 81.3. The fall of rain is the least throughout the year,

expressed by 0.835 inches. The evaporation advances to 3.503 inches. The easterly exceeds the westerly wind by one-third of a day, its duration being $5\frac{1}{3}$ days. June commences, the first month of summer, and has acquired a great relative increase in the dew point. The mean maximum temperature is 67.5° , the mean minimum 52.8° , medium 60.2° . The mean dew point 52.8° . After separating in the three preceding months, the dew point and temperature of the night again approach, and are expressed by the same numbers, while the degree of dryness diminishes to 7.4° , and the degree of saturation to .751, the mean of the least being .498. The weight of moisture in a cubic foot of air 4.400 grains. The sun's direct influence is at its height, being 27.7° , the greatest power 42° . It will be observed, that while the sun's greatest influence is poured upon this month, the mean temperature of the atmosphere in shade goes on increasing in the two following months. Can it be accounted for, in some respect, by the appropriation of radiant heat to the increasing amount of aqueous vapour in July and August, the dew point rising to 55° in these months, or is it entirely dependant upon astronomical causes? The mean pressure is 30.041 inches, the range being 1.10. The fall of rain is 2.215 inches. The evaporation is at its height, the mean quantity 3.690 inches. The South West prevails over the others, and blows nine days.

The mean temperature attains its highest degree in July and is expressed by 61.9° . This increase is made by the nights chiefly, the mean minimum amounting to 55.0° , whilst the maximum is 68.7° . We find that the dew point attains also to its greatest height, and approaches nearer to the mean of the air; it is 55.0° leaving a degree of dryness of 7.4° , and increasing the hygrometric state to .817. The mean minimum in point of saturation is .520, thus commencing to rise over the lowest means of the year. The weight of moisture is the greatest in this month, and is raised to 4.858 grains in a cubic foot. The fall of rain is 3.142, and the evaporation 3.069 inches. The

mean weight of the atmosphere 30.081 inches, and mean of the greatest range 0.91 inches. Of the various winds, the South West exceeds in duration blowing for a term of $9\frac{2}{3}$ days. The sun's heat is 26.0° , and greatest force 39° ; showing a decrease in its power from last month. Observations* show how much the stratum of air over the earth, and in contact with it, has its temperature influenced by the state of the sky during night, a clear and unclouded state favouring terrestrial radiation, while it is impeded by an opposite condition. In this and the next month, the accumulation of moisture in the air is large, the dew point is high, circumstances which greatly contribute to a particular interruption of the free passage of heat into space, and hence the comparatively dull state of the atmosphere by night in these months, will tend to keep up the temperature which otherwise would be reduced. The difference between the mean temperature of July and this month, is trifling, the mean of August being 61.7° . The nights are even warmer than those of last month, by half a degree, the mean minimum amounting to 55.5° ; the main maximum is 67.9° ; the mean dew point 54.6° ; the mean degree of dryness 7.1° ; the degree of saturation is .791, and the mean degree of least saturation, .555. The weight of moisture in a cubic foot of air 4.706; the sun exerts diminishing power, being 24.1° ; the greatest force 40° . The mean fall of rain does not exceed 1.712 inches, while the evaporation is 3.503 inches. The barometer marks as a mean, 30.093 inches, while its mean range is one of the least in the year, not exceeding 0.88 inches. The South West and South East winds blow an equal number of days, each having in the average, a duration of $5\frac{2}{3}$ days. The almost uniform temperature of the last three months is now interrupted, and we find a marked decline of the mean in September, which expresses only 56.8° . This decrease takes place chiefly in the days, as the difference in the mean of the nights is not so decided.

* Well's beautiful and instructive Essay on the Formation of Dew.

The mean minimum 51.7° , mean maximum, 61.8° , medium 56.8° ; the mean dew point does not observe the same ratio in decrease, it keeps up to 52.4° ; the degree of dryness is consequently small, not averaging over 4.4° . The degree of saturation increases very perceptibly, and amounts to $.875$; its least degree of saturation will also be high on the scale $.601$; the weight of moisture in a cubic foot of air will now diminish with the declining temperature to 4.435 grains. The sun's beams will also be less felt, though not in its real progressive decline, as generally a clouded or overcast state of the sky, with rains or high winds, bearing along vehicles of vapour, and occurring in the autumnal equinox, will oppose the diffusion of radiant heat on the ground, and will not permit the mean to rise as high as in the comparatively clear and cloudless month of October. The mean amount of rain deposited will be 3.453 inches, while the power of evaporation will be greatly diminished and not exceed 1.710 inches; the mean pressure may be considered as the lowest of the year, not averaging more than 29.741 inches, while the mean of the greatest range is only 1.106 inches; the South West wind blows $6\frac{1}{3}$ days.

The mean temperature of October declines in the same proportion, in which the day is most concerned, the mean maximum 55.9° ; the mean minimum 47.2° ; medium 51.6° ; the nights do not lose their warmth in the same progress as the days; the relation between the dew point and temperature is less, mean dew point 47.8° ; mean degree of dryness 3.8° ; mean amount of saturation, $.873$; and mean of the least degree of saturation $.636$. The cubic foot of air contains 3.783 grains of moisture; radiant heat 21.3° , greatest power 34° . Quantity of precipitation 2.270 , and of evaporation 1.519 inches; mean pressure 29.847 inches, and mean range 1.28 inches. The North West wind blows more frequently than those from the other points, averaging seven days.

In November the nightly and daily temperature decreases in equal proportions; the medium 46.8° , is made up of mean mini-

mum, 42.7° , mean maximum 50.8° ; the mean dew point has fallen several degrees, and stands at 43.9 ; the highest degree of saturation does not occur in this month averaging $.896$; the degree of dryness is 2.9° , and the mean degree of least saturation $.774$; the radiant heat falls to 11.9° , its greatest power being 28° ; the largest fall of rain is in this month, amounting to 5.382 inches, and the evaporation is a fifth part, not exceeding 1.020 inches. The average height of mercurial column 29.855 inches, and the mean of the greatest range 1.31 inches; the North West wind blows $7\frac{1}{2}$ days; December is but a fraction over the temperature of January, and is 3° less than November. The mean is 43.8° ; the air approaches here nearest to the point of saturation; the degree of dryness is only 2.5° ; the degree of saturation amounts to $.922$ in the mean, and the mean of the least degree of saturation is also greater than in any other month, being equal to $.788$. The sun's influence is 7.7° , the greatest power 24° ; the mean pressure is 30.137 inches, and the mean of the greatest range 1.46 inches; fall of rain 2.939 inches; the evaporation averages a sum less than that of any other month, not exceeding 0.651 inches. Northerly winds blow $5\frac{2}{3}$ days, exceeding the southerly by only two-thirds of a day. The weight of moisture in a cubic foot of air is 3.094 grains.

For a general purpose, the preceding summary of our climate would perhaps be available, but as the subject has been taken up in a medical view, it becomes indispensable to the right understanding of it that we should enter into all particulars bearing upon it. For this reason I will proceed first to the distribution of heat, and consider it in its different relations to the days, months, and seasons, and will contrast with the observations of this town those registered in a few other places, and chiefly such as have been noted during the corresponding years, which are thus marked (†).

*	Mean Annual Temperature.	Mean Temperature of the Seasons.			
		Winter.	Spring.	Summer.	Autumn.
† Cove	51.6	43.9	49.4	61.3	51.7
† Cork	50.3	41.8	47.3	62.1	49.9
Dublin . .	49.1	39.2	47.3	59.5	50.0
Penzance	51.8	44.0	49.6	60.2	53.3
† London . .	50.3	39.9	46.6	64.3	51.1
† Bedford	50.4	39.4	47.6	63.6	51.1
† Liverpool	50.0	40.8	46.4	61.6	50.3

As contrasted with Cork, which is distant only eight miles inland to the North West of this town, the distribution of heat throughout the season shows, in a striking manner, the advantages derived both from local situation, and the presence of a large body of water. In the winter it is warmer by 2.2° ; this superiority still continues in spring, whilst it is cooler by nearly one degree in summer, but again rising over Cork in autumn. The nights of summer are warmer by 1.7° than those of the city, but the days of the latter give the increase to the mean, being 3° warmer than those of Cove. In the other seasons, both the nights and days in this place are warmer. I have selected Penzance as possessing the mildest and most equable climate on the

* The Cork calculations are drawn from a private journal, and that kept at the Royal Cork Institution, by Mr. Connell. The Dublin are extracted from Dr. Clarke's useful work on Climate, &c. Those of Penzance, from a paper written by Dr. Forbes, and published in the Provincial Transactions, vol. ii. London, from the Athenæum. The Bedford observations are published by Captain Smith, in the United Service Journal. To Mr. Abraham I am indebted for the Liverpool register, kept by him in Lord-street.

South or South West coast of England, of which we have published reports, and in comparing it in the above table with Cove it will be seen, although placed in a higher latitude, that the latter enjoys a winter and spring equal in temperature to the former, though it sinks by 1.6° in autumn, and advances one degree in summer. In winter we are 4.7° warmer than Dublin, and in spring 2.1° , this decrease between both places chiefly taking place in May. Our summers are also warmer, and autumn is nearly 2° over that city. I am induced to compare with our climate that of Bedford, as from its inland situation, and consequently to a great degree removed from those influences which the presence of a large body of water exercises, it offers a certain contrast, not only in the distribution of heat, but likewise in its hygrometric condition, with places situated near the coast. It has an annual temperature 1.2° less than ours; the winter and spring are, as will be expected, much colder, the former by 4.5° , the latter, by 1.8 . The same causes which, in these two seasons affect the temperature, will contribute to an advance over us in summer, and hence we find that inland town 2.3° warmer. In autumn it descends to that of Cove. Liverpool is 3.1° colder in winter; this difference is observed also in spring; in summer it is a fraction warmer, and in autumn, 1.4° colder. The environs of Liverpool are from 1 to 2° colder in the early months and autumn season, than the town. The winters of London are 4° colder than those of Cove; Spring is 2.8° , and autumn, 0.6° ; but the summers are warmer by 3.0° . The nights and days of this last season are warmer than with us, the former, by 2.2° ; the latter, by 4.1° .

The climate of the west coast of England differs, according to Dr. Clarke, from the south in several particulars; we shall also find a difference between it and the south of Ireland, and for this purpose will select Bristol, whose mean annual temperature at 8 A.M. is 46.6. The mean temperature of Cove, deduced from observations taken between 8 and 9 A.M. is 51.0° being 4.4° higher. Bristol, in winter, is 34.6° ; spring, 46.0 ;

a mean which is principally indebted to May, as the temperature rapidly rises in this month over that of April, which is 41.2, the former acquiring 13.2°. In summer, 61.3°, and autumn 46.2°. Winter of Cove, 43.1°, spring 48.2°, summer, 61.3°, and autumn 51.4°. From this comparison, making allowance for the different years of observation of course, we are considerably warmer in winter, less so in spring, of equal temperature in summer, and again resuming the advance in autumn. In order to show the ranges of temperature in the seasons as well as the difference between winter and summer, in the respective places above selected, and this climate, and from which its peculiarities will be further understood, this table is subjoined.

	Difference of Mean of Winter and Summer.	Winter and Spring.	Spring and Summer.	Summer and Autumn.	Autumn and Winter.
Cove . . .	17.4	5.5	11.9	9.6	7.8
Cork . . .	20.3	5.5	14.8	12.2	8.1
Dublin . .	20.3	8.1	12.2	9.5	11.8
Penzance . .	18.7	5.6	10.6	6.9	9.3
London . .	24.4	6.7	17.7	13.2	11.2
Bedford . .	24.2	8.2	16.0	12.5	11.7
Liverpool .	20.8	5.6	15.2	11.3	9.5
Cove, (9, A. M.)	18.2	5.1	13.1	9.9	8.3
Bristol, (8, A. M.)	26.7	11.4	15.3	15.1	11.6

Here Cove and Penzance exhibit more uniformity throughout the seasons than the rest, and the mean of these two climates drawn from the above ranges, differ only 0.6 of a degree. When we come to examine the distribution of heat of each month in Cove, and contrast it with other situations, we find a

superiority in those months of greatest importance over them, and only equalled by the Cornwall climate. A glance at the mean monthly temperature of only two of the above places will exhibit this.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
Penzance	42.5	43.5	46.4	48.5	54.0	58.5	61.2	60.9	57.6	53.7	48.8	46.1
Liverpool	38.9	41.9	42.1	45.2	51.9	60.5	62.1	62.1	57.1	52.1	41.7	41.7
Cove .	43.5	44.4	45.4	48.2	54.7	60.2	61.9	61.7	56.8	51.6	46.8	43.8

Had I been able to give the corresponding month of December in Penzance and Cove, the discrepancy would not appear so great perhaps as above.* In January, Cove is 2.6° warmer than Cork, and in March, 4.1° . In May we are $\frac{1}{2}^{\circ}$ cooler, and in July, 1.1° cooler. In January, London is 4.4° , and in March, 3.3° colder than Cove, while, in July, it is 3.9° warmer. In January, Cove is 5.0° , and March, 3.1° warmer than Bedford, being in July 2.8° less. February, in this town, is 4.0° , and March, 5.7° warmer than Edinburgh. At Chichester, on the South coast, January is 7.1° , and March, 4.1° colder, and at Sidmouth, on the South West coast of England, the former month is 7.2° , and the latter only 0.3° less than this climate. This difference in two of the most important months of the year, between the respective places, is well worthy of attention. In following further the distribution of heat and its variations, I will place in opposition to the results obtained here, those of Penzance, for the reason already assigned.

* My friend Mr. Henwood, Secretary to the Royal Geological Society of Cornwall, informs me that no register, since the death of Mr. E. C. Giddy, in 1833, has been kept at Penzance.

Mean of the Highest and Lowest Temperature of each Month.

	Jan.		Feb.		Mar.		April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.
Cove . .	53	30	54	33	59	31	61	33	69	38	73	46	76	48	74	48	70	43	63	35	57	34	54	30
Penzance	54	28	55	33	59	34	62	36	68	41	72	46	73	51	73	51	69	46	64	40	57	36	56	33

Mean Monthly Range of Temperature.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
Cove . . .	23	21	28	28	31	27	28	26	27	28	23	24
Penzance .	26	22	25	26	27	26	22	22	23	24	21	23

Mean Daily Range of Temperature for each Month.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
Cove . . .	6.2	7.9	11.1	12.3	11.6	14.6	13.7	12.4	10.1	8.7	7.7	5.4
Penzance* .	4	6	8	9	9	8	8	8	7	6	5	3

Mean Successive Daily Variation of Temperature for each Month.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
Cove . . .	2.9	2.3	2.3	2.1	1.6	1.6	3.1	3.1	1.8	2.3	2.4	2.6
Penzance .	3.7	3.2	3.1	2.5	2.0	1.6	1.4	1.2	2.5	3.0	3.1	4.4

* This is only the range of seven, A.M., and two, P.M., being much less than that of twenty-four hours, from which the Cove means are taken.

Mean of the Greatest Rise of Temperature in the Successive Daily Variations.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
Cove .	8.0	4.3	7.3	5.6	5.5	3.3	5.3	5.1	6.0	6.6	8.1	6.0
Penzance	10.5	10.0	10.5	8.0	5.6	5.6	5.0	4.6	9.0	9.9	11.2	12.5

Difference of the Mean Temperature of the Warmest and Coldest Months.

Cove	18.4
Penzance	18.7

Difference of the Mean Temperature of Successive Months.

	January and February.	February and March.	March and April.	April and May.	May and June.	June and July.	July and August.	August and September.	September and October.	October and November.	November and December.	December and January.
Cove .	0.9	0.9	2.9	6.5	5.5	1.7	0.2	4.9	5.2	4.8	3.0	0.3
Penzance	1.0	2.9	2.1	5.5	4.5	2.7	0.3	3.3	3.9	4.9	2.7	3.6

Absolute Range of the Thermometer.

	Highest Extreme.	Lowest Extreme.	Range.
Cove (three years) . .	78°	27°	51°
Penzance (12 years) .	84°	19°	65°

Mean Range of the Thermometer.

	Annual Range.	Monthly Range.	Daily Range.
Cove	47°	26°	10.1°
Penzance	49°	24°	6.7° *

* This is only the range of seven, A. M., and two, P. M., being much less than that of twenty-four hours, from which the Cove means are taken.

Variation of Temperature of Successive Days for the whole Year.

	Mean Variation.	Mean of the Highest Variation.	Greatest Variation.
Cove	2.2°	6.0°	8.5°
Penzance . . .	2.6°	8.4°	10.0°

These results show in a particular manner, the equability of temperature of this place. In its ranges and variations there is a decided superiority over that climate, with which it is alone compared in several respects, and which are obtained by local advantages. This peculiarity will appear striking when compared with other places in a few leading results. The mean of lowest temperature of Cork, for example, in January is 26°, and the range of same month, 27°; the lowest of March, 26.5°, and the range 30°; the annual range 57°, mean monthly, 32.7° mean daily 13.9°; highest extreme, 85°, lowest, 23°; leaving an absolute range of the corresponding years of 62°, which in Cove is but 51°. In Liverpool the mean of the lowest temperature of Junuary is 26°, and the range 25°; in March 30°, and the range 31°; the mean annual range, 54°, monthly, 26°; highest extreme temperature for the corresponding years, 80°, lowest 23°; leaving a range of 57°; difference of the mean temperature of warmest and coldest months, 23.2°, and mean difference of successive months, 3.9°.

At Bedford the difference of the mean of warmest and coldest months is, 26°.2, and that of the successive months 4.4°, the absolute highest temperature 86°, lowest 22°, range 64°. At St. Helen's in the Island of Jersey,* and so situated as to feel all those influences which promote equability of climate, the absolute range of five years is 62; the highest temperature being 88°, and lowest 26°; the main variation of successive months is 3.6°; and the mean daily range 11.7, which is 1.6° more than Cove. On considering the different degrees of elevation at which the town is built, we will expect to find, be-

* Observations on the Topography &c. of the Island of Jersey, by G. S. Hooper M. D., 1837.

tween the lowest and highest situations, a difference of temperature both in winter and summer. It may be said, that this difference amounts to 2° in the former season, and about the same in the latter. There are days in winter and spring, when the depression of the thermometer will be greater than this which expresses the mean, and again, days when the variation is trifling; the same will be observed in the summer. If the comparison be carried to the interior of the island, we find that the mean difference of the nights of the colder months is 2.4° below the town. In December the mean of the nights in the country is 36.2° , of the town 39.4° ; and in March the former is 34.5° , the latter 36.9° ; these variations will depend upon relative position. In noticing the temperature of the external air, it is of some importance to observe the relation which that of an apartment shall bear to it in the two first seasons, for while southern aspect and northern shelter, contribute to a mild climate, the same will tend to raise the temperature in-doors to a certain height. A registering thermometer (day and night) suspended in a sitting room with southern aspect, and kept without a fire during last winter and spring, gave the mean of the former 51.8° , and the latter 55.4° ; the mean daily range of the latter being only 4.4° , and of the former 2.5° ; this temperature differs in each season about 8° from the external air. The advantages of a regulated temperature, when confinement in-doors is desirable, are too well known to need observation; without the use of a stove or fire, the air of the apartment is comfortable, and to raise it to a height of 60° , beyond which it is seldom necessary to go, the introduction of the former does not produce such a relative disproportion between the heat and moisture of the atmosphere, as where such a natural elevation of temperature could not be obtained.

From the topographical position of the Great Island, and the effect of the different winds, and their variability, we will find an almost daily vacillation of the mercury. Atmospheric pressure is in a constant state of fluctuation, and seldom

beyond two or three days have I known the barometer to remain steady ; even this term of duration is of rather rare occurrence, so that each day presents marked, and frequently very great changes. With the interior of this country I have not the means of comparison, but from our position on the sea-coast, should judge that the variations of pressure would be observed greater with us. I shall, therefore, be indebted again to Dr. Forbes' able paper, quoted before for a relative exhibition of the barometer's movements, by which it will appear that in Cove it observes a greater height in the means of the maximum, does not sink so low as at Penzance, and has a smaller range. This marks strongly the greater force of moist and depressing winds on that coast than here. That these fluctuations exert an influence over the sensations and functions of the living body is quite evident, and as relating to the subject of this paper, I have introduced them here.

	Cove.	Penzance.
Mean pressure	29.99	29.61
Mean of the highest pressure of each month	30.52	30.09
Mean of the lowest ditto ditto	29.30	29.08
Absolute maximum	30.84	30.42
Absolute minimum	28.90	28.28
Absolute range	1.94	2.14
Mean annual range	0.12	1.68
Mean monthly range	0.26	1.00

During the three years of observation, the mercury attained its maximum with a North East wind, and fell to its minimum with a South East wind. The following table will show the number of days the barometer indicated the highest and lowest pressure during the respective winds.

All Easterly Points.		All Westerly Points.		North.		South.	
Highest.	Lowest.	Highest.	Lowest.	Highest.	Lowest.	Highest.	Lowest.
17	4	6	28	10	2		2

A greater density of the atmosphere exists with easterly than with any other winds, and next to the easterly are the northerly.

The importance of inquiring into the hygrometrical constitution of the atmosphere is readily admitted, while experiments have not been as generally made as the subject demanded, hence, in drawing up reports of climate, we look in vain for data upon which to found our knowledge of its aqueous contents, attention having been paid in particular to the temperature of the air. The actual amount of aqueous vapour entering into the composition of the aerial element; its relations to that of the temperature; the distribution throughout the months and seasons, and days; its variations and obedience to the winds, require as intimate an examination as any other meteorological subject. Many of the effects upon the living body, which are attributed to temperature alone, will, I am persuaded, have their origin in particular relations of aqueous vapour to the air, and so altering the medium by which it is surrounded, create a variety of sensations. We know that a sense of chilliness is conveyed by an alteration in the hygrometric quality, while no reduction whatever takes place in the temperature of the atmosphere surrounding us. In health and in disease its influence is felt, and that climate, in which a man shall enjoy active health, will, on certain morbid conditions of his frame being developed, exert an influence proceeding from variations of moisture. The effects produced in passing from a temperate to a tropical climate, as well as in a lesser degree, those experienced in our own, from winter or spring to summer, show what influence is relatively exercised by the proportion of moisture. In the spring and autumn, when the dry-winds blow, though their mean temperature shall not differ from those of opposite points, we witness impressions on vegetable and animal bodies alone, arising from their hygrometric state. So sensitive are invalids to variations in the watery contents of the air, that I have frequently seen a reduction of only one degree in the dew-point occasion cough and local irritation of the larynx, when suffering from bronchial or tubercular disease, when

a depression of seven degrees of temperature would not have produced the same. We have constant illustration of the action of dry and moist air on the functions of the skin and lungs. Every medical man will have witnessed an altered hygrometrical state displaying its influence over the symptoms of several diseases. This, therefore, becomes not only an interesting subject, but one of practical benefit, a knowledge of which leads to a clearer understanding of the efficacy of climate, and its adaptation to particular cases. The actual amount of moisture entering into the air of Cove will be greater than in the interior, or in places removed to any distance from the coast, provided inland sources of vapour shall be cut off. With no part of Ireland, in this respect, can I compare it, as I am not acquainted with any hygrometric observations which have been or are being taken in this country. In England, amongst the many places from which we have meteorological reports, only two containing hygrometrical observations of any length have come under my notice—London* and Bedford; I shall therefore compare this climate with them.

Mean Monthly State of Saturation.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Total Means.
Cove .	.894	.862	.775	.749	.736	.751	.817	.791	.875	.873	.896	.922	.829
London	934	877	863	809	764	765	791	817	845	868	929	932	841

Mean Monthly State of Saturation at Cove and Bedford at 3 P.M.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Total Means.
Cove .	.894	.835	.747	.704	.647	.693	.697	.697	.765	.818	.868	.863	.764
Bedford	803	714	673	567	450	431	414	425	508	605	698	743	586

* Daniell's Essay on the Climate of London.

Difference between the Mean Dew Point of Successive Months.

	January and February.	February and March.	March and April.	April and May.	May and June.	June and July.	July and August.	August and September.	September and October.	October and November.	November and December.	December and January.	Annual Means.
Cove .	0.1	2.2	2.5	5.2	6.9	2.2	0.4	2.2	4.6	3.9	2.6	1.0	2.8
London	0.6	4.1	4.5	2.6	4.6	2.9	0.8	3.0	7.5	4.3	2.9	3.3	3.4

Mean State of Saturation of the Seasons.

	Winter.	Spring.	Summer.	Autumn.
Cove . .	.892	.753	.786	.881
London .	.881	.812	.791	.881

Mean State of Saturation at Cove and Bedford at 3 P. M.

	Winter.	Spring.	Summer.	Autumn.
Cove . .	.864	.699	.695	.817
Bedford .	753	563	423	603

Mean Daily Range of Dew Point.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Annual Means.
Cove .	1.1	1.1	1.2	0.8	0.7	0.7	.8	1.0	0.5	0.6	1.4	0.8	0.9
London	1.7	0.7	1.0	1.1	0.8	1.0	1.1	1.2	3.3	1.4	1.3	1.6	1.3

Absolute Range of Dew Point.

	Highest Extreme.	Lowest Extreme.	Range.
Cove,	68°	23°	45°
London, . . .	70°	11°	59°

Absolute Range of Saturation at Cove and Bedford at 3 P. M.

	Highest Extreme.	Lowest Extreme.	Range.
Cove, . . .	1.000	491	.509
Bedford,905	.299	.606

On referring to the results we are at first rather surprised at the higher hygrometric state of the air of London than of Cove, but when the relations of both are considered in respect to the sources of aqueous vapour, we can understand how this amount should be so high in a densely crowded city from which exhalations will be constantly going on. The standard temperature of the dew point is less by 2° in London, than in Cove; and hence the actual quantity of humidity composing the basis of our atmosphere is greater in the latter place, the increasing temperature of the air producing relative degrees of electricity. The mean temperature of the dew point follows that of the minimum of each month, and does not partake of the great movements which distinguish the maximum; hence, in winter, the point of saturation is high, and diminishes in spring and summer, and rises again in autumn; so that compared with the mean of the lowest temperature, both would follow the same course, while with the mean of twenty-four hours there would be a considerable departure from it. The larger quantity of moisture in the air near the sea than in an inland climate, is here strikingly exhibited, and the decrease in the latter place as the sun advances; and again, its increase as the temperature declines, further illustrate what has been said. The large amount of moisture in July and August, as compared with an inland place, will explain why, when the atmosphere is calm in the low parts of this town, a sensation of oppression and langour shall be experienced by some, while the temperature on the finest day will be less, as we have already seen, than in other situations, but whose proportion of vapour is smaller. The equability which characterizes our temperature appears in a remarkable manner in the distribution of moisture. The

difference between those months whose degree of saturation is the greatest and least amounts at Bedford to .372, and at Cove to .241. The mean monthly range of dew point at London is 27° , at Cove 20° . The above table relatively and comparatively shows this quality. In December the highest mean range of dew point is observed, and in July the least. The mean of the highest dew point occurs in August, and of the lowest in March. The mean successive daily variation of dew point 2.9, and greatest rise 10.8. The monthly order in which these successive daily variations are felt is March, January, February, April, November, December, May, June, July, August, October, and September the least. In the seasons the greatest difference is observed between spring and summer, the least between winter and spring. Inland the least variation takes place between spring and summer. The hygrometric character of our climate, here recorded, combined with the degree of temperature which it enjoys, produces a mildness and steadiness surpassed by none, if equalled by any in the kingdom. In connexion with this part of the subject we shall, before proceeding to the influence of winds on the temperature and hygrometric state of our climate, consider the deposition of rain. From our situation, from the exposure to those winds which are the vehicles of great bodies of moisture, and from the quantity of vapour entering into the constitution of our atmosphere, it will be thought that this town will be much more than most other places visited with heavy rains. We are always disposed to attach an idea of frequent appearances of deposition to those spots which are placed beside great bodies of water, without stopping to examine the matter comparatively, and hence a climate is pronounced very rainy and very damp. The degree of accuracy pertaining to such opinions can only be pointed out by the results of careful observation. Having in the first part of this paper given the monthly fall of rain, I will not in the first column introduce it, but give the amount of deposition of each month in the places with which I am desirous to compare the results obtained here.

Mean Monthly Fall of Rain.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
Cork, (1 year,) }	1.877	4.909	.791	1.544	1.215	1.215	3.266	1.446	1.917	4.642	3.854	3.119
Liverpool, (3 years,) }	4.183	2.243	2.263	1.606	1.489	2.553	4.126	1.666	2.696	2.852	3.393	1.956
Bedford, (3 years,) }	2.075	2.035	2.453	1.992	2.673	1.890	1.687	1.782	3.172	3.065	2.403	0.869
Penzance, (12 years,) }	3.7	3.3	3.4	2.9	2.9	2.4	3.1	3.8	3.7	4.6	4.9	5.5

Distribution of Rain in the Seasons.

	Winter.	Spring.	Summer.	Autumn.
Cove	10.565	4.060	7.069	11.605
Cork	9.896	3.750	5.927	10.413
Liverpool	8.382	4.758	8.345	8.942
Bedford	4.979	7.118	5.359	8.640
Penzance	12.5	9.2	9.3	13.2

Number of Days in each Month on which Rain falls.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
Cove . .	11	14	10	10	8	13	11	10	15	12	14	10
Cork . .	17	24	6	15	12	11	14	10	10	22	23	17
Penzance	15	14	14	13	12	11	13	14	13	16	18	21

Number of Days on which Rain falls in each Season.

	Winter.	Spring.	Summer.	Autumn.
Cove . . .	35	28	34	41
Cork . . .	58	33	35	55
Penzance .	50	40	39	48

The annual fall of rain at Cove, as already stated, is 33.299 inches; at Cork, one year, 29.786 inches; Liverpool, seven years, 32.849 inches; Bedford, 26.096 inches; and Penzance, 44.7 inches. From this summary, Penzance exceeds Cove by one-third of the whole quantity deposited; as much does not fall at Cork as here. The inland situation of Bedford gives less, and Liverpool amounts to nearly the same. Now, we would expect to find a number of rainy days in those places corresponding to the fall of rain, but a reference to the above table, as well as the yearly amount, will show that not in proportion to the deposition, will be the days of rain. The annual number of rainy days in Cove is 138; in Cork, 181; Bedford, 141; Penzance, 179; and Clifton, 149. In winter and spring the rainy days of Penzance are nearly double the number of Cove, and the fall of rain, as would be presumed, greater, particularly so in spring. I wish to state, as it is desirable in every statement professing to give an account of a climate, that the fall of .004 part of an inch constitutes a rainy day. A rainy day is so designated also in Cork, by the same small deposit. This rain may fall in any part of the twenty-four hours. The greatest fall of rain takes place in November, but the lines of temperature and dew point approach nearest in December. The same disproportion which we have observed between the quantity of rain and number of rainy days in comparative places, we shall also find between the month or seasons of the same place; for instance, in Cork the number of rainy days in spring is greater

than in autumn, though the number of inches is less in the former than in the latter season. In the month of March, in Cove, the days are as many as in January, while the fall of rain is one-half less. This want of proportion arises from a difference in duration and degree of intensity of the fluid's descent. Accompanying the rain is sometimes a foggy state of the air, which contributes more than the former to raise the hygrometer to saturation. In the ordinary descent of a shower or continued rain, the air placed between the vesicular vapour is not saturated; the more subtile diffusion of the minute vesicles and greater accumulation in a given space is necessary to produce the effect. Such a condition of the atmosphere must, necessarily, from its very small capacity for moisture, and increased conducting power for heat, act more sensibly on the exhaling systems, producing chilliness, langour, or oppression, according to the season of the year, state of the health or accompanying temperature. Of any two seasons, this accompanying fogginess occurs more frequently in summer and autumn. In winter it is observed more than in spring, the mean frequency being in spring four times, and winter thirteen times. With southerly winds in winter, which sweep over great surfaces of water, and reach us loaded with vapour, this state of the air is noticed. In summer, more frequently, the effect is produced by a change from sea to land wind in the fore or after-noon. The number of foggy days, those when the pluviometer has received no appreciable fall of rain, is few, being but four in winter, six in spring, four in summer, and five in autumn. This fog appearing at any period of the twenty-four hours is noted for an entire day. They are generally the precursors of fine weather. The South wind in winter, moving slowly over the ocean acquires increased humidity, which arriving in an atmosphere previously cooled, and meeting a reduction in its temperature, is separated, and becomes visible vapour. At other times before this, or a South East, or a South West wind reaches land, the fog is already rolled along the sea, and rising

over the shore, covers in an extensive veil the entire coast. Though this kind of weather is not marked by a deposition of rain, yet the appearance of moisture is spread over vegetables, house-tops, and other bodies, in sufficient quantity to create dampness. In winter there is more of the character of mistiness in these fogs than in summer. I do not mean here those fogs which in spring and autumn are observed inland, and which fill up the valleys; they are the effect of terrestrial radiation, and their density will depend upon the comparative quantity of moisture previously existing in the atmosphere. It seldom happens that the rain continues to fall all day, so that there are hours of brightness and dryness in the greater number of rainy days. The number of days on which rain falls all through, is very small compared to the whole; during the entire period of those observations, there were only fifty-two, viz.:—fifteen in winter, two in spring, fifteen in summer, and twenty in autumn. Exercise may, therefore, be taken in the open air, almost every day in such parts as are sheltered from strong winds. In spring the weather is showery, and though the sky shall be frequently overcast in winter, and rains fall heavily, yet the intervals of sunshine, and the days of continual calm and brightness, are truly delightful. From the inclined position of the town the rain flows freely off, and in a short time after the heaviest shower, when the sun bursts forth, the walks become quickly dry. From the hygrometric characters of our atmosphere, we learn that the capacity for evaporized fluid is comparatively small, when contrasted with inland reports. The whole amount of water raised in the form of vapour falls short by nearly a seventh of the sum precipitated; at Bedford, on the contrary, the evaporation exceeds by one-sixth the amount of rain, though it is calculated from the ordinary covered vessel. Cove, in summer, gives 10.262 inches; the same season at Bedford, 14.833 inches. In spring, in the former place, the force of evaporation is equal to 8.344, and in the latter, 7.745 inches. In autumn it diminishes, and is, on the coast, 4.249, while inland it is 6.482 inches.

In winter they approximate closer to the former, being 2.758, and the latter 3.210 inches. The summer and autumn seasons, inland, outstrip those of the coast, while the mean amount of winter and spring is the same in both places. Had the results obtained with a similarly constructed vessel to that used at Bedford been here contrasted, they would show a great disproportion between these places.

Having in the preceding pages endeavoured to point out the characters of our climate with respect to the sun's impression upon our latitude, without considering the various currents in which our aerial sea moves, and without whose agency the important changes, all of which tend to a general good in the great economy of creation, would not occur, we shall now pass to the notice of winds, and examine them in relation both to temperature and moisture, as they affect us. Without an understanding of these agents we cannot form a just conception of any situation. A mean temperature will be made up in various ways, and a winter and spring may give a degree of heat at first view favourably high, but which, on further inquiry, will be found to be counteracted by unfavourable winds. For the more accurate comprehension of this interesting and important subject, I have thought it necessary to enter into this method of viewing it; indeed, observations deduced from the winds for a sufficient length of time, will bring us very closely to the results obtained by the ordinary methods. The mean temperature of all the winds, for the short period employed here, is only a very small fraction less than the mean already stated; and the mean dew point of same differs only half a degree from that before expressed. The comparative advantages of a climate can only be estimated in this way, as by it we can readily judge of the various influences which are exercised over living bodies, and show how and when those influences are felt. From the position of Cove, noticed in the first part of this paper, it will be presumed that winds from the colder quarters are comparatively little felt, while to the warmer it is exposed, and this is proved by ob-

ervation. From the North, North East, and North West, it is protected, not to the same degree from the East and West, the general direction of the hills being according to these points, but which, at either end of the town, swell into prominent ridges, and present to their course a certain opposition; and to the South, South East, and South West, it is quite open.

Table showing the Mean Temperature and Dew Point of each Wind in the respective Months.

	S.		S. W.		W.		N. W.		N.		N. E.		E.		S. E.	
	Temperature.	Dew point.	Temperature.	Dew Point.	Temperature.	Dew Point.	Temperature.	Dew Point.	Temperature.	Dew Point.	Temperature.	Dew Point.	Temperature.	Dew Point.	Temperature.	Dew Point.
January . . .	44.7	42.8	46.8	44.2	47.3	44.4	41.2	38.0	37.6	34.9	37.9	35.4	40.9	37.4	43.4	41.9
February . . .	47.4	45.4	46.1	43.6	44.6	40.0	43.7	38.9	43.2	39.2	42.0	37.8	47.0	45.0	46.5	45.4
March . . .	44.7	43.1	46.0	44.0	44.4	38.6	43.6	36.8	42.9	35.6	43.3	37.3	44.1	37.4	49.2	42.4
April . . .	50.3	45.9	50.4	46.4	50.7	43.6	47.6	40.1	45.7	37.1	44.3	35.5	44.0	38.0	46.2	43.0
May . . .	53.8	48.1	55.4	49.1	54.5	46.3	52.8	43.9	52.3	43.0	53.2	42.4	56.5	43.9	54.3	48.3
June . . .	61.6	56.0	60.9	54.3	59.5	51.3	59.9	50.3	60.9	50.4	63.9	54.9	61.4	52.9	58.9	54.5
July . . .	62.7	60.0	62.8	56.2	60.7	53.9	60.3	51.6	61.2	51.9	66.0	57.0	67.1	57.9	63.5	57.8
August . . .	63.4	60.2	63.9	57.1	60.5	54.7	60.8	53.5	58.9	51.8	63.5	55.8	63.3	57.2	64.0	58.0
September . . .	58.0	55.6	57.8	54.6	55.7	50.6	54.4	48.6	57.3	48.9	57.1	53.4	59.8	54.7	61.9	58.1
October . . .	54.2	52.5	54.3	51.1	51.3	48.2	48.9	44.6	45.7	41.5	51.4	48.6	52.7	49.9	55.1	52.5
November . . .	51.2	51.0	50.2	48.1	47.0	44.4	44.5	41.2	42.6	39.7	42.9	37.9	43.2	41.0	47.9	46.7
December . . .	49.1	47.6	49.0	46.7	46.8	44.6	41.2	41.4	38.9	36.0	37.5	34.2	38.6	35.3	46.3	44.3

From the observations of morning, afternoon, and night, this table is constructed; the minimum temperature is not here introduced, though it has been considered in the yearly mean, yet the results very closely correspond. The temperature of all the Southerly Winds, and those to which we have observed the town is exposed, is 53.4° . That of Easterly and Westerly Winds, 51.7° , and of all the Northerly Winds, 49.8° . Thus we find that the mean of the year of all the winds as they affect Cove, varies very little, the difference of the mean of the warm-

est and coldest being only 3.6° . The Westerly and Easterly are the same, differ but by a fraction, and are only 1.7° below the Southerly. This remarkable uniformity in the average of the year is striking, and exhibits the peculiarities of our climate. When the mean dew point is considered, the same features are presented. All the Southerly Winds are 49.9° , the Northerly, 46.9° , thus differing 3° . The mean dew-point of the Westerly and Easterly, 46.3 , the former differing but 0.7° from the latter, and both being only 3.3° dryer than the Southerly. On referring to the above table, we find that the means are made up of winds having a high summer heat, and a comparatively low winter temperature, while others are more uniform ; thus the N. E. and E. are the warmest in summer, while the Southerly are the coolest ; and in February the N.E. is 5.4° colder than the Southerly of same month. This brings us to consider them in regard to their influence in the different seasons. The degree of dryness which accompanies each wind will here be observed. It will have a regular course throughout the year, and increase as we pass from those winds which blow over the sea to the land winds. Were we to describe by segments of circles the progress of each wind's degree of dryness, a small curve would express the march of Southerly dryness from January to December. This will increase in the South West Wind, become still more developed with the West, go on increasing with the North West, attain its maximum with the North and North East, sink again with the East, and mark nearly the same curve with the South East as the South, the degree of dryness in the former being a degree more than in the latter ; we shall accordingly find the hygrometric state of the Southerly Wind higher than any of the others, and the North and North East lower. The degree of saturation of Southerly Wind is, .911, South West .872, of the West .836, North West .813, North .805, North East .810, East, .832 and South East .887. I am aware that to arrive at a correct deduction of the wind's temperature, the observations of many years are required, for a wind may more

frequently blow where the sun's temperature shall have declined, than in the midday, and hence its mean shall express a disproportion to the wind from another point; the reverse shall also obtain; but yet our observations will approximate, I should hope, to the results of more continued inquiry, and enable us to set forth the advantages of this climate. In winter and in spring the effect of winds over our temperature and moisture is more observable than in the two later seasons, and in winter, when the sun is still low, the influence which they exercise over these properties of our air is very remarkable; in spring it is less observable.

Mean Temperature and Dew Point of the Winds in the different Seasons.

	S.		S. W.		W.		N. W.		N.		N. E.		E.		S. E.	
	Temperature.	Dew Point.	Temperature.	Dew Point.	Temperature.	Dew Point.	Temperature.	Dew Point.	Temperature.	Dew Point.	Temperature.	Dew Point.	Temperature.	Dew Point.	Temperature.	Dew Point.
Winter	47.1	45.2	47.3	44.8	46.2	43.0	43.3	39.1	40.0	36.3	39.8	35.8	42.2	39.9	45.4	43.8
Spring	49.6	45.7	50.6	46.5	49.8	42.8	47.7	39.6	46.9	38.6	46.9	38.4	47.2	39.7	49.9	45.5
Summer	62.5	58.7	62.5	55.8	60.2	53.3	60.3	51.8	60.3	55.3	64.4	55.9	63.9	56.0	61.1	56.7
Autumn	54.4	53.0	54.1	51.2	51.3	47.7	49.2	44.8	48.5	43.3	50.4	46.6	51.9	48.1	51.9	50.5

In winter we shall here observe that the South and Westerly Winds are 6° warmer than the North and Easterly, while in spring they are only 3°. Now the difference between these winds in Penzance, according to Dr. Forbes, both in winter and spring, is from 6° to 8°; so much more are the effects of the cold winds felt at that climate in spring than here. The South and West winds are warm and soft, while the East and North are cold and piercing. In situations exposed to the free influence of their action on the sensations, this difference is at once felt, and is productive in susceptible constitutions of the most serious consequences. Wherever an extreme variation between the

range of these winds is found, that climate cannot be a suitable one in many diseases. Hence the importance of a situation which to a certain degree protects against this evil, and shelters in the most trying season of the year the delicate and the susceptible frame.

In spring we find the temperature of the North East and North Winds nearly as high as the Southerly, and such a proportion of aqueous vapour accompanying them as to render their action comparatively mild. In estimating the effect of the East and North East Wind upon living bodies, we will find that where it is most decided, the proportion of moisture to the temperature is small. After passing, in their progress to these countries, over an immense territory, they visit many parts of them in a high state of dryness; they seize with an avidity every particle of moisture exhaled from every surface they touch; the vegetable life is paralyzed, the exhalents are constricted, the circulating vessels are shrivelled up, and the freshness and fullness, which but a few days before were developed in spring's returning heat, wither under the blast. The hygrometric state of the wind will explain this. On the human frame the effects are striking; in vigour and in health they are stimulant, in debility or disease they are the same, but the issue is different, in the former, giving tone to the system, in the latter, exciting congestions or inflammations. These properties of a very dry air are readily explained. In a moderate degree a dry air is beneficial, according to the state of health, and the requisites of the case, but we know how necessary it is to guard against the effects of Eastern Winds in the early part of the year. With the interior of the South East part of England I can compare the temperature of the cold winds, and find that at Bedford the mean temperature, at three, P. M., of the Northerly Winds of winter, 38.3° ; of the North East, 36.5° , and of the East, 38.7° . The mean temperature of the same winds at Cove is each 5° higher. In spring the same winds are 4° higher in Cove than Bedford; the hygrometric state of these winds will also be much lower in the latter than the

former place. From the direction given to the East and North East Winds by the chain of mountains which run from the North toward the South of England, and which has been noticed by Drs. Garrick and Symonds, in a paper on the Medical Topography of Bristol,* I should think that the South and South West coast must feel their effects very much, but am at a loss, from the want of data, to compare the depression of temperature and moisture, as there experienced, with our own. To show more clearly the protection which Cove enjoys from the winds above named, I will here contrast the results with Cork in December and March, drawn from observations taken at nine, A. M. Mean temperature of Northerly Winds in December at Cork, 31.5° ; of North East, 31.0 , and of East, 33.6 ; the Northerly Winds at Cove are 3.9° ; the North East, 4.5° , and the East, 4.5° higher in the same month. The Northerly Winds of March are 3.0° ; the North East, 4° , and the East, 3.3° higher likewise in Cove than Cork. To any one familiar with atmospheric phenomena, the capriciousness in the movement of winds must have drawn attention. The great variability which is displayed by them must alter the qualities of heat and moisture in our air, which, before the change shall be indicated, on the thermometer in particular, is immediately detected by our own feelings. Invariably these properties change along with the change of wind, as from South to North, or the intermediate points of West and East. Occasionally, during the continuance of South or South West Winds, and chiefly if it blows a gale, the thermometer rises gradually, at other times rapidly, and thus bringing additional heat by night as well as by day; we find the difference between these two periods as low oftentimes as 1° . The sun's influence on such days, is quite shut out in winter by the loaded state of the atmosphere, and the variation consequently small: on a change of wind, suppose to the North West, the watery vapour is dissipated, or descends

* Transactions of the Provincial Medical and Surgical Association, vol. ii.

in rain, the air is cleared, the sun shines out, the temperature in shade falls, and the daily variations are felt. Now these opposite states of the air are felt in acute and chronic disease. To increase the proportion of moisture to the temperature in one case, and the temperature to the moisture in the other state of the air, are the indications. Raising the temperature of an apartment, whose air contains too large a proportion of vapour, while the particular state of the external atmosphere prevails, will have the effect of decreasing in a given space, the amount of that vapour ; and while, on the other hand, when the degree of dryness is felt, by the simple introduction of steam from boiling water the dryness is decreased, and, as for example, in those cases where irritation and cough shall have been provoked by the change, the air rendered unirritating. The latter is more easily accomplished than the former, as it may not be advisable to raise the temperature to such an height, which, though it shall decrease the proportionate moisture, may exert too debilitating an effect, while in the latter, the temperature of the external air being also less, we can with more advantage create the artificial medium. The strongest gales felt by us, blow from the South East, and end generally in heavy falls of rain in the colder months ; when from the Southerly points the winds are mild, as is commonly the case, a softness, a warmth, and a feeling of comfort, are experienced both in winter and spring, which, added to a bright sky, and cheerful and ever varying prospect, give a peculiar character to the seasons. The position of the town, which exposes it to gales from the Southerly points, on the other hand, protects from all those proceeding from the colder quarters. When such winds prevail, the air of the town is undisturbed, a calmness and a brightness reign, the waters of the harbour rest tranquilly beside the shore, the mildness of a summer's day is in the very depth of winter enjoyed ; and not until the clouds are observed to fly swiftly across the heavens, and the distant surface of the waters before us ruffled and thrown into motion, are we reminded that an inclement

season of the year is then prevailing. This description of weather is often noticed. In the relative duration of the winds we will find that the South West exceeds the others. We shall there contrast the annual duration of each with Penzance.

Mean annual Duration of each Month.

	S.	S. W.	W.	N. W.	N.	N. E.	E.	S. E.	
Cove . . .	33	74	53	71	44	28	25	37	Days.
Penzance .	45	58	39	76	36	38	37	36	Days.

In this table we find the East and North East Winds twenty-two days less in duration throughout the year, than at Penzance. The East are to the West Winds, as one to two, the North exceed the South, the North East are to North West, as one to three, and the South East to the South West, as one to two in Cove. The order in which the North East and East Winds prevail in the respective months, beginning with that in which their greatest duration is observed, is March, August, April, July, January, May, December, June, September, February, and least in November. The average total of winds from cold points, exceeds that from warm points in Penzance, while the latter in Cove exceeds the former. In Liverpool we find the annual duration of the North East Winds, thirty-seven days, and of North West 123 days. The prevalence of South West, and Southerly Winds, is demonstrated by their effects upon trees and under shrubs along the Southern shore. Where those winds can freely pass onward, the branches are directed to the point opposite to that whence they proceed, and a North Easterly inclination given. The same cause also checks the development of foliage on those distorted branches. The “*pinus sylvestris*” and the “*mespilus oxycantha*” appear principally to exhibit this change. In distributing through the respective seasons the annual proportion of winds, we shall find them as follows.

	S.	S. W.	W.	N.W.	N.	N. E.	E.	S. E.
Winter	9	17	13	18	14	8	2	9
Spring.	8	14	16	17	13	11	8	4
Summer	7	25	11	15	8	5	9	13
Autumn	9	18	13	21	9	4	6	11

East and North East Winds prevail most in spring, next in summer, and in autumn and winter the least. A comparison with penzance will give an idea of the relative frequency of easterly winds in both places. In that climate they prevail thirty four days in winter, forty-four in spring, thirty-two in summer, and thirty-eight in autumn : a reference to the above table will exhibit nineteen in winter, twenty-three in spring, twenty-seven in summer, and twenty-one days in autumn ; thus making a great disparity, which is chiefly found in the winter and spring seasons.

From what has been already said of the comparative hygrometric state of each wind, we will be prepared to find most rain deposited where those which pass over the ocean blow, and diminished as they come from a land direction ; so that rain is generally expected with Southern gales, while it is only occasional from the opposite points, and very sparing in quantity.

In the warmer seasons the rainy tendency is conteredacted by the increased temperature. With the South Wind there falls 7.651 inches in depth, with the South West, 12.281, the West, 3.618 ; the North West, 1.752 ; the North, 0.350 ; the North East, 0.223 ; the East, 0.519 ; and the South East, 6.868 inches. We find here that though the East wind blows one-half the number of days, during the whole year, that the West prevails, the depth of rain is one-sixth less, and though the North prevails one-fourth more than the South, yet the amount in the former is as one to thirty-six. As we should expect, the proportion of rain to the number of days is greater with a South than a South.

West Wind, or a South East Wind. It will also be observed that the same, though in a greater degree, obtains with a South West, compared with the West. The different distances from the ocean which have been before noticed, will explain this. After having quitted the Atlantic, and mounted over the Western shore, the great clouds of vapour are arrested by the high lands lying West of us, are deprived of part of their humidity, and reach this climate in a lower degree of moisture. The South West Winds have also to pass over high land, but of less extent, and meet a proportionate diminution of vapour. The South, least of all, meets obstruction, and hence the higher amount of rain. The North Wind, after passing over the Northern Sea, has the whole extent of our country to traverse before it arrives on the south coast, and will, in that course, be deprived of much of its aqueous contents: the capacity for vapour, however, is much less than the winds above named. The North East and East Winds have very small sources of vapour, compared with the others.

From the mildness of our winters and springs, the number of days of *frost* and *snow* will be small. The steadiness with which the temperature is kept up, by the causes mentioned in the paper, will account for the exemption from them, and although there is included in the average one of the severest seasons that has been known to visit this place for upwards of twenty years, yet the mean number of days on which snow fell, is but seven, and the mean number of times on which the thermometer fell to and below freezing, is nine. Snow, when it falls heavily, which is very unusual, never remains more than a few hours, and a common appearance exhibited in a snow shower, is the solution of the flakes in the warm air, long before they reach the ground. Indeed, of late years, snow is so seldom seen, that when it does visit the town it is looked at with astonishment. When we compare the results with Cork and the vicinity of London, we find the average number of days on which the mercury sinks to and below the freezing point,

amounts to 44 in the former, and at Chiswick* to 70 in the corresponding years. These show a remarkable resistance in Cove to the depressing influence of cold. The same observations apply to hail showers ; they chiefly occur in the spring months, and their effects are transitory.

Thunder visits us so seldom that it is scarcely worth recording ; for the whole term of observation it has been heard two days in winter, two in spring, and four in summer. It may be here worthy of remark, that while in Penzance winter is the season in which it has been chiefly noticed, with us it has been most in summer. I regret that to these few observations I am not at present able to add any upon the electrical conditions of the air. Whoever has watched the approach of a thunder storm must observe and feel the direct influence over the animal functions which is then displayed. The causes of disease will have a closer connexion with the electric variations than we now comprehend ; while, therefore, so much benefit has been conferred upon medical science by what has already been done in meteorological inquiries, let us hope that this branch of them will receive that attention which its value and importance appear to demand.

Botany.—In the vegetable bodies which live on our soil, and which are not indigenous to it, we will find the effects of our climate exhibited. We can much better judge of the character of an atmosphere by these means, than such as are derived from instrumental observation. The various properties already detailed will receive a confirmation and illustration from vegetable existences, and be the best natural tests we can employ. I will here enumerate a few plants, which, with us, grow in the open air, and which, elsewhere, will be found to require in-door protection at some particular season.

Agave americana.

Amaryllis revoluta.

Aloysia citriodora.

Antholyza zoninia.

* Philosophical Magazine.

Buddlœa globosa.	Ixia crocata.
Calla Œthiopica.	Lachenalia tricolor.
Cineraria aurita.	Melianthus major.
Chrysanthemum Indicum.	Mimulus viscosus.
Cyclamen coum.	Myrtus (varieties of).
Dahlia (varieties of).	Mesembryanthemum (species of).
Dracocephalum canariense.	Oxalis caprina.
Fuchsia globosa.	Rubus rosæfolius.
Fuchsia coccinea.	Salvia cærulea.
Fuschia gracilis.	Sophora microphylla.
Geranium (several species).	
Hemerocallis japonica.	

During our winters vegetation is strong; the fields are covered in healthy verdure, and many of our native plants blossom nearly all the season through. The indigenous botany of this island is very interesting, and presents some rather rare specimens. The “*mentha rotundifolia*,” the “*marrubium vulgare*,” “*leonurus cardiaca*,” “*antirrhinum orontium*,” and many others, both in the phanerogamic and cryptogamic divisions of great interest are met here. I do not think it necessary to enter here into any particular catalogue of these plants, suffice it to say, that the botanist will find ample field of inquiry and profit in their pursuit. In the sheltered parts of the town, and even in the interior of the island, the myrtle, amongst the exotics, flourishes uninjured, and bears a profusion of blossoms to the latest month of the year.

The admiration of plants amongst the poorer classes is remarkable; there is scarcely a habitation, however wretched, in whose window is not observed bending toward the light and shew, a “*geranium*,” a “*mesembryanthemum*,” a plant of “*mimulus viscosus*,” or an “*agave* :” these, in habitations not in the neatest order or cleanliest part of the place, bespeak a taste for an innocent pleasure which draws at once the attention. In the summer months, though there shall have been little rain,

yet vegetation goes on rapidly from the amount of vapour in the atmosphere, and which is appropriated through the cuticular pores to the uses of the plant. Hence the foliage both of trees and vegetables is often remarked to be largely developed. This degree of moisture in the air is well shown by the practice of suspending young plants of the “*agave Americana*” in a room where the growth of the leaves is very great.

On the zoology of this island I have nothing particular to offer. It would have given me the utmost satisfaction had I been able to lay before the profession the climate of other parts of the South of Ireland, as contrasted with that which I have attempted to describe; but however desirous the object was, it could not be accomplished from the impossibility of obtaining reports. To have in particular compared Mallow, which is situated about twenty-six miles North West and inland, would have placed both climates in an advantageous light, and shown what diseases would derive a benefit from either, and what particular seasons were best adapted. We are to regret the want of observations, as they render such an important inquiry impracticable, though the situation, as contrasted with one nearer to the sea, will enable us to judge of some leading differences. The temperature of Mallow will in the summer months be warmer, and in the winter months colder, than Cove. The hygrometric state of the air will in the mean of the year be less; in one or two of the winter months it perhaps will not differ much from the proportion of dryness observed here, which is presumed from the comparison of an inland town with which Cove has been in the preceding part of this paper compared; but as the sun advances, its springs will be dryer, and its summers still more so than Cove; the autumn will begin to rise in its degree of moisture till it reaches again its maximum in winter. The range of temperature and of moisture, in all particulars, will be greater. From its natural situation it will not possess that equability of climate which is only felt the nearer we approach the sea. The same causes which tend to

diminish its equability, will permit the fall of the minimum temperature in the colder seasons. Frost will consequently prevail more.

From what has been said of the conformation of the hill upon which the town is built, it will be perceived that there are other most eligible building sites, so that the particular climate may be extended beyond the present limits. I cannot conclude this part of the subject without remarking, that had the natural beauties of this place been properly estimated, had art contributed to develope and bring forward the local advantages, and had modern improvements displayed their taste, it would have become one of the most attractive spots in the kingdom, and its character of climate greatly enhanced.

The climate, which we have endeavoured briefly to illustrate, may be classed with those of the South West of England and of France, in its effects upon disease. Than some of these places it will perhaps be more capable of application to a larger number of cases, for from what has already been observed, the degree of moisture is less. To appreciate fully the advantages of a climate, a most essential part in the investigation of them is a right knowledge of the degree of watery vapour to the temperature. By this we learn what influence it will exert over one disease, or one form of the same disease more than another, and upon what fair grounds of success we can recommend this or that residence. The limits of this paper will not allow me to enter upon the morbid history of this place; I will therefore briefly state, and will reserve particulars for further consideration, those cases which will derive a benefit from a residence here. We must not forget that a climate is either preventive, remedial, or palliative, under the particular circumstances attending a case.

In dyspepsia, dependant upon an irritable state of the mucous membrane, this air is serviceable. In that form of it having its origin in a congested state of the vessels, or from nervous causes, little benefit will be derived; indeed it will

decidedly disagree in general. Some uterine affections, as dysmenorrhœa, will derive advantage, the physical properties of the atmosphere contributing to relax the system, and promote secretion. Tone and vigour, have, by the combination of sea bathing, and equability of temperature, been given to the chlorotic frame, and the strumous habit so often accompanying and characterizing that disease corrected. Of rheumatism, where we find such a susceptibility to every variation of heat and moisture, and where of course, a climate subject less than another to extreme ranges, is desirable, moreover, where the hygrometric quality contributes to give a softness and mildness, remarkable in the warmer months, I can say that many cases are benefited.

The forms of asthma, which are least likely to derive advantage, are those in which the secretion from the bronchial membrane is copious ; the opposite cases, for reasons sufficiently obvious, are those to which this climate has been applied with the most marked beneficial result. In affections of the larynx accompanied with diminished secretion, or that condition of the vessels productive of it, the action is salutary. In organic disease of the heart, in dilatation with or without hypertrophy, and morbid engagement of the aortic valves, many of the pectoral symptoms accompanying these are considerably alleviated, and particularly the symptomatic bronchial spasm. When bronchitis is not attended with copious secretion, depending upon a relaxed state of the mucous membrane, this air is serviceable. Some of the months will exercise this beneficial influence more than others ; those for example in which the largest amount of moisture enters into the composition of the atmosphere, will not have the same effect as those in which the contrary occurs. The month of October has appeared to me to have a decidedly beneficial influence on even the chronic bronchites, with rather copious expectoration, and the same result has been observed in April and March. In that deranged state of the general health, which has been so ably described

by Dr. Clarke, and which constitutes tubercular cachexy, a residence in the winter and spring will contribute to alter and amend the constitution. In the first symptoms of tubercular deposition, and before the frame and functions seem to be brought directly under the influence of the disease, the most marked benefit has been obtained. When the disease of consumption has progressed farther, we cannot look for so decided a change, symptoms will be moderated and relieved, the health partially improved, and with much care and watchfulness, life prolonged. All cases of tubercular disease of the lungs, whether in their first, second, or third stages, receive a benefit, but in the too last it is too often transitory. When tubercular deposit has gone on so as to engage a large portion of the lung, when it is in its progress to softening, or when a cavity already exists, we cannot expect climate to work impossibilities. Too often are patients visiting this spot in whom, while the accents of hope fall from their lips, and prospects of lengthened days are vividly painted before them, disease has made its certain progress, and every word spoken sounds the sufferer's knell, and mocks his delusive prospects. A timely removal of a patient to any favoured climate is of paramount importance, as in the early stages of disease hygienic and therapeutic measures can be more satisfactorily applied. We witness the improvement which takes place by a change of air, in every case which presents, but the constitution in many has been brought too deeply under the disease to acquire a permanent benefit. Of the influence of the climate of Cove upon such Dr. Stokes remarks,* "Of course, as in all cases of the kind, the good effects of the climate are seen more in the temporary improvement of the health of patients, than in their final or permanent cure. Such, however, is the penalty which all places of the sort must pay for

* See Dr. Stokes' highly practical and valuable work on Diseases of the Chest, —a work which every man must prize who values his profession, and has the benefit of his patient at heart.

their celebrity." We need scarcely observe how strong therefore the necessity is, whenever a change of air is requisite, to apply its preventive or remedial influence at the earliest declaration of symptoms.

ART. V.—*Anatomy of Cowper's Glands in the Female.* By
J. R. TAYLOR, Assistant-Surgeon, 58th Regiment.

IN Mr. Guthrie's lately published Lectures on the Diseases of the Bladder and Urethra, is to be found, I believe, the only description of these glands contained in the works of modern authors, who have treated of the anatomy of the perinæum. Many, indeed, have remarked certain large mucous follicles at the orifice of the vagina, under the name of Cowper's glands, but true glandular bodies, with ducts, analagous in structure and situation to those so called in the male, are not, I think, described in any of the anatomical or obstetrical works, ordinarily in the hands of students. Mr. Guthrie's description of these glands is very brief; I shall therefore take the liberty, in the first place, of introducing it here, and I shall then add an account of the dissection of these glands, which, with the accompanying drawing, will, it is hoped, serve to remove the doubts and mistakes, still pretty general, concerning the existence and situation of these glands in the female.

Speaking of Cowper's glands in the male, in Lecture IV., Mr. Guthrie's remarks: "They appear to be sexual glands, and not connected with the excretion of urine, inasmuch as they are to be found in the female in a similar situation, with regard to the fascia, as in the male. Mr. Taylor, who made the greater part of these dissections, has shown them on each side in this preparation, with the excretory ducts opening into the vagina. He thought he had made a discovery, for, although Dr. William Hunter, in his great work on the gravid uterus, has shown the orifice of the duct, he considered it as a follicle only. The

older anatomists were aware, however, of there being a gland in this situation, before they knew there were any of a similar kind in the male.

De Graaf mentioned, and Cowper notices them in his *Anatomy of the Human Body*, when he was acquainted with those in the male, which now bear his name. Since making the dissections above referred to, I have found that Munro, in his "*Anatomy*," also gives a brief description of these glands. From his time however they appear to have been altogether overlooked, or if Cowper's glands in the female have been spoken of, it has been understood that mucous follicles were the structures alluded to. This error, I observe, still continues, notwithstanding the remarks just quoted from Mr. Guthrie's lectures, and as it is owing probably to the locality of the glands not having been very perfectly described, I am induced to think that a few words on the mode of dissection for them, and the accompanying drawing explanatory of their situation, may not be considered superfluous.

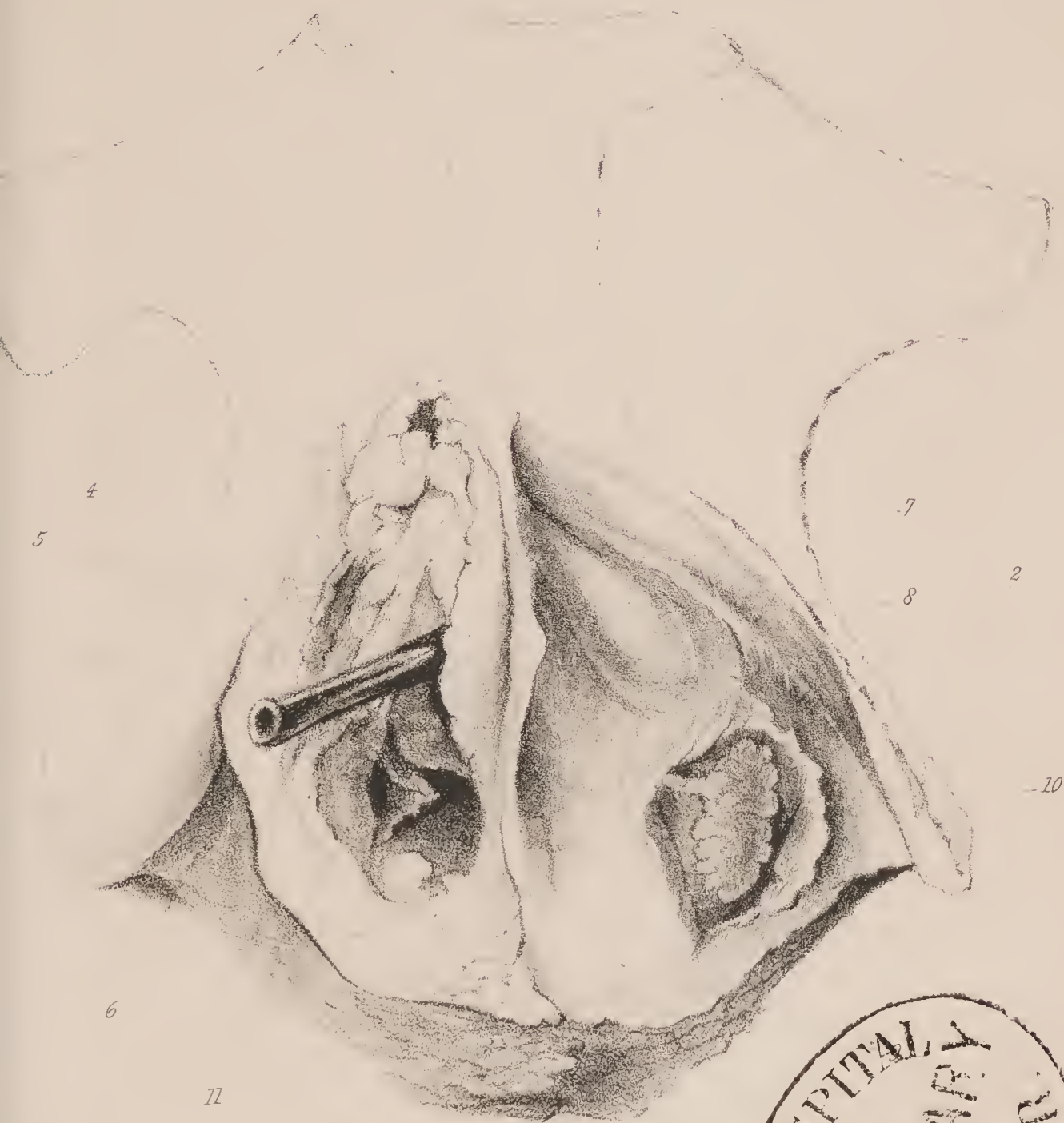
To commence the dissection of Cowper's glands in the female, the integuments, fat, and fascia forming the labia majora, may in the first place be removed, so as to expose the sphincter vaginae. This muscle being then carefully dissected off, the following parts are brought into view; just within the rami of the pubis the corpora cavernosa are seen passing upwards, forwards, and inwards, to form the clitoris. Immediately anterior, and below each corpus cavernosum, and extending in the same direction, is an oval flattened protuberance of a bluish-purple colour.(8) These protuberances may I think be very properly termed semi-bulbs of the corpus spongiosum; for besides resembling that part of the male in their internal structure, they are anteriorly united and continued forwards under the corpora cavernosa, to assist in forming the clitoris; it is also behind and below each semi-bulb that a Cowper's gland is to be found situated.

But to point out a still further resemblance of these semi-bulbs to the bulb in the male, it is to be remarked that the latter is also formed of two symmetrical portions, with a septum more or less perfect, and invariably existing, though not generally described. For these reasons it seems to me that we are justified in considering these protuberances of erectile tissue as semi-bulbs of the corpus spongiosum, which, like the halves of the corpus cavernosum, are particularly separated in the female, whilst, in the male, they are placed in close apposition. To return to the dissection of the glands—they are in this stage of it concealed by the anterior layer of the deep perineal fascia, which here, as in the male, passes in front of them on to the corpus spongiosum. From their firm granular structure, they may, however, be generally felt behind and below the semi-bulbs, and a careful reflection of the fascia in this situation brings them at once into view. They are of a flattened, oval form, and considerably larger than those of the male; they are placed with their long diameters from above downwards. The duct, which is about an inch long, issues from the anterior and inferior margin of each gland, passes upwards and inwards, and then opens into a large lacuna on the lateral surface of the vagina, a short distance within the os externum.

EXPLANATION OF THE DRAWING.

1. External surface of the symphysis pubis.
- 2, 2. Outline of the rami of the ischia and pubes.
3. Anus.
4. Glans clitoridis.
5. The nymphæ.
6. Blow-pipe in the urethra.
7. Corpus cavernosum of the left side.
8. Semi-bulb of the corpus spongiosum of the same side.
9. Anterior layer of the deep perineal fascia, but reflected off Cowper's gland.

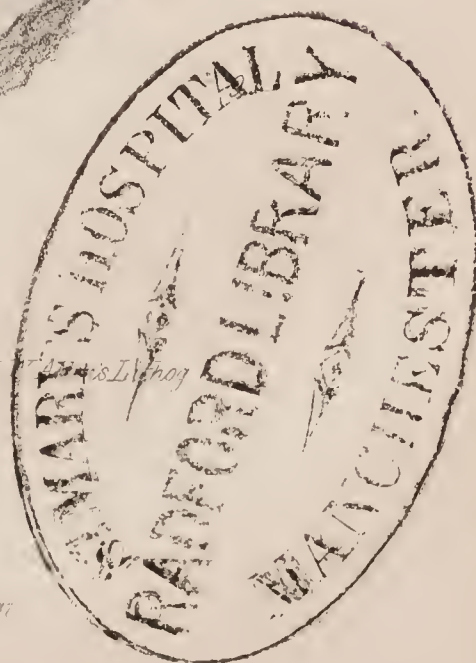
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Hodges and Smith Dublin
1858



10. Cowper's gland of the left side cleared of the fascia and cellular membrane, in which it will be found imbedded.

11. A bristle in the orifice of the duct of Cowper's gland, opening into a lacuna on the right side of the vagina.

ART. VI.—*Some Observations on Fever*. By HENRY KENNEDY, A. B.

[Read before the Medico-Chirurgical Society, December 9th 1837.]

To any one reflecting on the subject, there is nothing more remarkable in the whole history of medicine, than the fact of the different, I may say, opposite opinions of medical men of equal renown, as to the treatment or cause of diseases apparently similar: the varied treatment of puerperal fever or syphilis, will bear me out in this assertion, as also the numerous opinions held as to the cause of fever.

It is now some years since Broussais promulgated the opinion, that inflammation of the intestinal mucous membrane and glands was the cause of fever, and even at the present day the leading pathologists of the French school, Andral and Louis, hold a similar though somewhat modified opinion: while in Great Britain the fact is denied, and it is put beyond any manner of doubt, that intestinal inflammation is not a necessary cause of fever, or in other words, that fever may exist without this lesion. While my mind was in a state of suspense, from the conflicting opinions just alluded to, an opportunity occurred of observing fever, both at Geneva and Paris, and what I was informed was the common disease of the country: to my surprise I found, that the fever of the continent was different in many particulars from the typhus of this country: here then was some solution of the difficulty, and it struck me at once, that by following it up, the apparent contradictions might be reconciled. Since that time, now more than two years since, my opportunities for observing fever have been uninterrupted,

and I now feel convinced, that the fevers of the two countries are of different types, and that typhus may in the great majority of instances be distinguished from the gastro-enteric fever of the French. Without further preface then I shall draw the attention of the Society to this subject.

It is to be understood, that the observations are in reference to the fever complicated with enteritis.

The countenance of the patient, in the earlier stages at least, was but little altered; as the disease advanced, however, rapid flushing, sometimes amounting to actual congestion, of one or both cheeks was very constant, the eyes became unusually bright, sometimes injected, and some degree of nervous agitation might be observed, especially about the nostrils: as might be inferred from these symptoms, the intellect was nearly perfect, the patient's answers were rational and distinct, though frequently slow, and raving, when it did occur, was mostly at night: deafness was present in but a slight degree; the patients in many instances slept even well. It is scarcely necessary to contrast these symptoms with the heavy, suffused eye, the twitchings of the muscles of the face, the impaired intellect, the deafness, and the want of sleep, all so commonly met with in typhous fever. When, however, the disease was about to end fatally, the countenance assumed that sunk look, which has been alluded to by authors. I do not here speak of the gradual loss of flesh which every one passing through an illness of any duration suffers from, more or less, but to a sudden sinking taking place within a certain twenty-four hours. In the majority of cases this symptom could be traced to diarrhoea, profuse sweating, or hemorrhage: in some few cases I was unable to trace it to any cause; it seems probable that it goes hand in hand with the commencement of the ulcerative process; and it may be remarked, that whether it occur early or late, in fever or other disease, it is of equally unfavourable omen. The respiration in most of the cases was *uniformly* rapid, so much so, as to induce me to make an accurate examination of the chest: scarcely a bronchitic rale, however, could be heard: this, in itself, was an im-

portant point, for it led to a more accurate examination of the viscera of the abdomen. This rapid respiration is the more remarkable, when we contrast it with the irregular cerebral respiration of typhous fever. The pulse offered nothing remarkable: in the greater number it was rapid, 120 to 130 in the minute: in many it scarcely deserved the name, being more an undulation than a distinct pulse, and it was frequently dicrotous without any hemorrhage. It is, however, worth remarking, that serious, and even fatal mischief was going on, when its range was between 96 and 108; I have not remarked it intermitting. To the state of the digestive apparatus particularly, attention should be paid, for on it appears to me to depend the true diagnosis of these two types of fever. It is natural, indeed, to suppose, if we have two forms of fever, the one complicated with organic lesion, and the other not, (and that the latter does exist is now certain,) that the difference between them arises from the superadded symptoms caused by the complication. In the greater number of cases, the tongue was thickly coated with a whitish fur, while it was moist at the point and edges, and this appearance was presented whether the case was mild or severe: instances indeed occurred, where it was dry and brown, but these were exceptions to the general rule, and were frequently caused by the patient sleeping with the mouth open, or by the use of opium: the stomach being engaged or not, had an unquestionable influence; if it were engaged, the tongue was very apt to be dry, and the thirst great; if it were not (and this was far the most common) this organ was moist, and thirst but little complained of. No instance occurred where the patient was unable to protrude the tongue steadily. The red colour of this organ has been very generally spoken of as indicative of intestinal disease; it holds good only in some cases: redness, however, at the very point, may, I believe, be much more depended on, than where the whole organ is equally red.

In a masterly report on fever, published some years since by Dr. Barker, at the time Physician to the Cork-street Hospital,

it is stated that no single symptom affords so much information as the state of the tongue : the remark holds good as regards genuine typhus, but certainly not when applied to fever complicated with enteritis. Cases of pure gastritis are not included in these remarks, for they afford probably the best examples of the tongue, being, as it were, burnt up ; in fact it would appear that the lower down inflammation is situated in the intestinal canal, the less the tongue is affected. Thus I have seen instances of intense inflammation of the rectum, and also many cases of dysentery where this organ was very nearly natural. It is scarcely necessary for me to contrast the moist red, and above all, steady tongue of the one fever, with the dry, brown, shrivelled one of the other, together with the inability of protruding it. Sore throat was rarely observed ; when, however, it was, it seemed to be a certain precursor of irritation lower down in the canal, and the fever was unusually heavy. Vomiting was little complained of : in one instance the patient threw up some black matter : this case ended fatally : thirst was not a characteristic symptom, nor was the desire of the patient more for cold than hot drinks, and on the whole, the stomach attracted very little attention. Diarrhœa showed itself in every variety : in some it appeared at the very commencement of the disease, and in not a few it could be traced to the patient having taken a quantity of epsom salts : these were all very bad cases, and frequently fatal : in the majority, however, the diarrhœa did not appear before the fifth or sixth day, when the nurse would state that the patient's bowels had been disturbed two or three times within an hour : what was rather curious, the diarrhœa would then sometimes cease, but this by no means implied that mischief was not going on : in others, again, the looseness continued till the last, whether the patient recovered or died : in three or four cases diarrhœa was not present, and yet they terminated fatally, and *post mortem* examination exhibited extensive ulceration of the ileum. It has been remarked that the colon being engaged or not, would account for the diarrhœa being present or absent ; it would

appear to be a rational explanation of the fact ; I think, however, it is very probable that the low state of the nervous energy of the intestine might also account for it.

The tendency to diarrhœa was in many cases very remarkable ; saline draughts, emollient injections, and even whey, were enough to bring on this state ; what was passed, was almost invariably of the bright yellow colour first described by Dr. Bright, as characteristic of inflammation of the ileum proceeding to ulceration. Tympanitis was commonly present in a slight degree ; it took place early in the disease, and was more constant than what is observed in typhous fever ; in this latter it generally shows itself late in the disease, comes on very rapidly, and is always present to a greater degree than I have seen it in the enteric fever. The abdomen was sometimes rigid, though in far the greater number it was soft and yielding ; it commonly conveyed the idea of fulness ; the patients, when asked, generally said they felt no pain, and it was only firm pressure which would cause them to complain or change countenance ; slight pressure in two or three cases, however, caused exquisite pain, and yet in one instance in particular, where an opportunity occurred of making a post mortem examination, no peritonitis was detected ; the ulceration, however, had nearly perforated all the coats of the intestine, and it appeared probable that the peritoneum had become morbidly sensitive from its contiguity to the diseased part ; when the patients did complain of pain of their own accord, it was curious to observe how constantly they directed attention to the right iliac fossa.

Gargouillement is a symptom upon which a good deal of stress has been laid by some of the French authors : it appears to me to merit very little confidence, for it may be detected in almost every patient, no matter what disease they are labouring under, who has been kept for a few days on slops. Calor mordax has also been mentioned : it is but rarely present, and I have not yet seen an instance of this, where it was exclusively confined

to the abdomen. Both symptoms are, however, to be borne in mind, as they may be of value where other more important ones are absent. Dr. Stokes, to whom we owe so much, has the credit of drawing the attention of the profession to abdominal pulsation, as diagnostic of intestinal inflammation. Not being acquainted with the fact till very lately, I have made no observations on the point: analogy, however, would strongly favour it. Petechiæ are by no means characteristic of enteric fever: when they could be detected, they were very indistinctly marked, and disappeared very rapidly, and long before the disease terminated, the skin had become perfectly clear. In an interesting letter published on this subject in the *Dublin Medical Journal*, by Dr. Lombard of Geneva, notice is taken of this fact, and it appears to me to be correct; and if petechiæ of a dark colour are set down as characteristic of typhus fever, the disease may almost be said to be unknown in France. A mottled state of skin, very like the mildest form of measles, did, however, present itself in the earlier stage of the disease. In a very few cases only were sudamina observed about the clavicles, but they present themselves in so many diseases, that no credit can be attached to them: I have seen them in bronchitis, in rheumatism, and I knew one instance where they were observed in tetanus.

The slight degree of subsultus, and in many cases its total absence in the enteric fever, is a symptom which affords a marked contrast to genuine typhus: in this latter, few, if any cases occur where subsultus is not present in a marked degree, while, in the former, even fatal cases occurred where this symptom was completely absent. In two instances, however, it was observed to affect the lower extremities slightly, when no other part of the body exhibited it: this will not appear strange when we consider that any irritation of the bowels is capable of producing spasms of the legs, as exemplified in constipation, or the more formidable disease of cholera. The varieties of temperature in the cases of enteric fever were remarkably sudden: in some the

feet would become quite cold, while, at the same time, the face would get flushed, and the head hot, whilst, again, the feet would be burning, and the face and breath both cold. It has often struck me, while observing these cases, that animal heat must depend, in part at least, on the healthy state of the intestinal canal : chronic disease of this part in the adult, but more particularly in the child, would help to confirm this opinion. The duration of this disease is much greater than that of typhus fever, but I shall speak of this more hereafter.

The foregoing symptoms are then those in which, as it appears to me, the fever met with in France differs from the fever of this country ; it may be as well briefly to recapitulate them.

The natural state of the countenance ; the bright eye ; the perfect intellect, amounting, in the worst cases, to but a slight degree of stupor ; the raving mostly at night ; the steady protrusion of the tongue, which was mostly moist and red at the very point ; the diarrhoea, with the characteristic yellow discharges ; the pain on pressing the ileo-cæcal region ; the absence of true petechiæ ; the very trifling degree of subsultus ; the variations of temperature ; the enjoyment of some degree of sleep, and the duration of the disease, are then the symptoms on which, it appears to me, a true diagnosis may be founded. From the above list it may be observed, that the difference between the two fevers consists in the great predominance of what are termed nervous symptoms in typhous fever, and their absence, or nearly so, in the other. It must be observed, that the above signs are spoken of relatively : thus, when it is stated that the countenance is natural, it is not to be understood to be perfectly so, but comparatively so, when contrasted with the countenance of a patient labouring under typhous fever.

On the pathology of this disease, I have nothing to offer, but must refer to the admirable statistical account given by Louis. One or two points, however, are to be observed : the

fever complicated with enteritis, and met with in this country seldom exhibits the malignant appearances on dissection, which are described as occurring on the Continent. I have not yet seen cases where pus has been found in the absorbents or mesenteric glands ; or where there has been an extreme degree of softening of the mucous membrane ; in fact, gastro-enteric fever may be said to be endemic in France, and to exhibit from time to time lesions of the very worst description, precisely as typhous fever, which is endemic here, now and then assumes a very bad type, but where post mortem examination discloses little if any lesion in the intestinal canal ; it is not, however, to be understood, that enteric fever does not occur epidemically in this country ; all I mean to say is, that it is rare. In the cases where an opportunity occurred of making an examination of the head, the brain was found unusually healthy ; whereas it is known, that in most cases of typhous fever, it exhibits some degree of lesion, and in a considerable number quite enough to cause the death of the patient.

Through the kindness of my friend Dr. Irvine, I had charge of the *Maison de Santé* for several weeks during the past autumn, and some cases of this fever then came under my care ; not, however, having any thing new to offer, I shall be as brief as possible.

When called upon to treat a case of this disease, the general points then to be kept in view are the following. That fever, complicated with gastro-enteritis, is mostly of a low type ; that symptoms of this lesion seldom make their appearance before the fifth or seventh day, and consequently our treatment previously should be expectant ; that the disease runs a much longer course than common typhus, three, four, and even five weeks being not at all uncommon, and therefore the patient's strength is to be supported from an early period ; that the patient is not to be considered safe because the diarrhœa may have ceased, or the appetite returned, for still fatal mischief may be going on ; and lastly, if there be no disease in the

chest or brain, and the fever is still continuing beyond the usual period, that enteric lesion may be suspected, and should be particularly sought for. The particular treatment may be divided into external and internal; the latter cannot be too simple, if it be thought necessary to check the diarrhœa; a combination of Dover's powder, rhubarb, and mercury, with chalk, is probably the best can be employed; or the Dover's powder alone is an excellent remedy; caution is however necessary in the use of such medicines. I have frequently seen the use of those just mentioned put a stop to the diarrhœa, only to be followed by bronchitis, and such indeed has happened to myself, but in no instance was it of a severe character; in some cases it was observed that both diseases became exceedingly mild, the inflammation being as it were divided between the bronchial and intestinal mucous membrane; it is always to be remembered, that diarrhœa is Nature's own way of removing this inflammation; in most of the cases therefore it is not to be interfered with, and in the worst should only be moderated, not checked. When purgatives are to be used, they should be of the mildest kind, such as manna, and rhubarb; injections are of the greatest service, even they should consist of little more than tepid water. There is one medicine which Dr. Graves has recommended in some stages of typhous fever, and which in this disease may also be administered with great benefit, the spirit of turpentine; it is scarcely necessary to say its use must be preceded by appropriate treatment, and when prescribed, it should be given in doses of ʒss. to a ʒi., every four hours, combined with mucilage and a little tincture of hyosciamus: it should not be given till the disease is advanced. It is very important to pay attention to the patient's drinks; all of the mucilaginous class are very useful, as also a mixture of lime water and new milk, or almond emulsion with a few drops of tincture of opium; when wine is necessary, it should be given largely diluted; it is scarcely necessary again to repeat, that mild nourishment must be given from an early stage of the

disease. The external treatment consists in the repeated application of leeches in small numbers, to the ileo-cæcal region; as a general rule the bites should be stopped when the leeches drop off, but the cupping glass may be frequently applied with great benefit. Poulticing is very grateful to the patient after the leeches, or even without them; they may consist of linseed meal, or scalded bran; the old remedy of the heated salt also gives great relief, and it has the advantage of not being moist. Some practitioners seem to think it necessary to prescribe for the tympanitis of fever; it appears to me that it would be just as philosophical to order bandaging to put a stop to the subsultus. The application of large blisters will sometimes put a stop to the disease, particularly if they be applied immediately after the leeching; they should not be left on longer than six hours, and are to be covered with silk paper. The use of flannel, keeping the feet warm, and frequent sponging of the body, are also of great importance; in fact, it has always appeared to me, that the external treatment was of more importance than the internal; the question may possibly arise, what should be done in case the disease run into ulceration, and the patient still hold out? The most rational answer to this question would probably be, the cautious removal of the patient to another ward or room, the internal use of nitrate of silver, and salivation by inunction.

I have now drawn the attention of the Society to a subject of some importance: in the first place being one of the complications of fever, it is most undoubtedly under the influence of medical treatment, and many cases may be saved, which, if left to themselves, would certainly be lost: whereas, fever, which is purely essential, is but too little under the control of medicine: in the next place, this complication is beyond any question, the most insidious which occurs in the fever of this country at least: stethoscopic examination will detect morbid lesion in the chest, while there are almost always symptoms present which

will direct attention to the brain, should it be engaged; but here the most extensive disorganization will be going on, and yet it is possible there may not be a single symptom which would lead the practitioner to suspect it. I have over and over again heard persons saying, that such and such fever patients would die of intestinal ulceration; they did indeed die, but the intestines were found free from disease; and again I have seen cases where *post mortem* examination was the first thing to declare that the patients had died of gastro-enteric disease; another reason, however, still remains, why the practitioner should pay particular attention to this subject. The sequelæ of fever have been very numerous these last two years, particularly parotid abscess, erysipelas, and the still more formidable disease of diffuse inflammation; and I think I have seen quite enough to enable me to state, that there exists some connexion between these sequelæ, and at least an irritable state of the intestines, that in fact they stand to each other in the relation of cause and effect: I have notes of several cases which would bear me out in this.*

In an essay on the subject of diffuse phlegmon from the masterly hand of Dupuytren, it is stated, that diarrhœa is one of the most deadly symptoms which can occur. The observations I have made would lead me to state, that the irritable state of the bowels precedes the appearance of this disease, as well as the other sequelæ to which allusion has been made. I know that sequelæ occur where the bowels appear to be healthy from first to last; but they are exceptions to the general rule: if this be established as a fact, it is scarcely necessary to repeat, how incumbent it is on the medical man to allay any irritation of the intestines occurring in fever.

* The following may be thought interesting. Two individuals, husband and wife, after having eaten a quantity of salad at dinner, were at the same moment seized with sickness of stomach and purging: they were admitted into hospital the following day: the husband passed through a severe attack of erysipelas of the head and face: the wife was attacked with diffuse inflammation, of which she died.

Before I conclude, I should state, that within this last week, and since this paper was written, a number of the American Journal of Medical Science fell into my hands, which contains the second part of a paper on typhous fever, by Dr. Gerhard of Philadelphia: in it he alludes to the difference between simple typhus, and the same complicated with enteric disease, and as far as his observations go, they are singularly similar to my own: in this respect I must consider myself peculiarly fortunate, for my experience, when compared with his, has been very limited: no single circumstance, however, could have occurred, which would more strongly support, or confirm the different observations put forward in this paper.

ART. VII.—*Summary of Mr. CARMICHAEL'S Views of the Venereal Cases in which Mercury is admissible or is not admissible.*

THIS paper owes its origin to a conversation I had a few days since with one of the physicians who so ably conduct this Journal. He expressed strongly a desire, that I should briefly put on paper the circumstances under which I employed mercury for the cure of venereal complaints, and stated his opinion, that such a paper, in the present unsettled state of the mind of the profession, would be attended with advantage. This suggestion has induced me to overcome my unwillingness to enter again upon this most fertile subject for disputation; and in complying with it, I shall perhaps add but little to what I have already published, as my practice in venereal complaints has not varied much during the last twenty-five years.

As I admit that mercury is an agent of great utility in some forms and stages of venereal diseases, when duly administered under sound pathological principles, and not blindly given as a specific, I cannot be deemed an anti-mercurialist. But I differ

widely, both in theory and practice, from the downright mercurialist. He gives mercury, *instanter*, for all forms and stages of venereal diseases, with the exception of gonorrhœa; and if he withholds it afterwards, it is because he finds that his patients are becoming worse under its use, and then he complaisantly calls these obdurate and unyielding symptoms, syphiloidal or mercurial, and relinquishes for a time the farther use of that mineral. I, on the contrary, reject mercury in all forms and stages of venereal complaints except true syphilis, taking Hunter's description of it for my guide; i. e. the hardened edge and base as indicating the primary sore, and the scaly eruption (from the commencement) as indicating the secondary or constitutional affection. And now I shall briefly detail the exceptions to this my general rule, and the circumstances under which I am induced to prescribe mercury; first premising, that I seldom or never exhibit it for the primary symptoms, which occasion the papular eruption, viz. excoriation of the glans and prepuce, and an ulcer in many instances better described by its negative than its positive characters, so much is it likely to be modified by neglect, irritation, mode of living, and constitution of the patient. The negative characters to which I allude, are its want of induration and phagedena; for its positive I must refer to my Essay, as it would occupy too much space here to detail them. This form of disease, perhaps, amounts at present to nine-tenths of the cases which occur in general practice. Others, for instance the primary phagedenic ulcer, and the constitutional symptoms, rupia, tubercles, &c. connected with it, are far more tedious and difficult of cure, and therefore it is, that we find the beds of hospitals filled in an over proportion with cases of this obstinate form of venereal disease.

1st. If cases of the simple primary ulcer of the papular venereal disease do not yield to rest, the antiphlogistic treatment, and astringent washes, after the third or fourth week, I usually give mercury in alterative doses, in the same manner and with

the same views as I would exhibit it for any indolent ulcer which is not venereal ; but this is seldom or never necessary.

2nd. When the papular and pustular eruptions become scaly, and obviously on the decline, in general not sooner than the fourth or fifth week, if not yielding satisfactorily to sarsaparilla and antimonials, I exhibit mercury in alterative doses, combined with sarsaparilla.

3rd. Whenever iritis occurs, I give mercury so as to excite its full effects upon the system.

4th. When nodes arise, which usually commence with inflammation of the periosteum, I also give mercury so as to produce its full effects ; and in the two last instances, it is exhibited on the principle, that there is no process so powerful in checking periostitis or inflammation of any membranous part, as mercurialization of the system.

It is here worth remarking, that in the course of a long practice, both public and private, I have never met with nodes as an attendant upon the papular eruption ; which in my mind, is an indication, that the poison which produces it is different from that which occasions the other forms of venereal eruption.

5th. In the phagedenic form of venereal disease, I may safely say, that I have always found, sooner or later, the exhibition of mercury prove to be injurious. For primary ulcers, invariably so, and the same may be observed while the eruption continues to present the form of rupia, or tubercles. But after the disease has existed for months or years, each succeeding crop of eruption, I have observed, has a tendency to change its character into that of scaly tubercles ; of which there are many excellent drawings in the museum of the Richmond Surgical Hospital, open to the inspection of all medical men. In this state, alterative doses of mercury may, *perhaps*, be of use ;—yet, of this I am very doubtful, for I have seen, even in this exhausted state of the disease, more relapses than perfect cures by mercury, exhibited either in full or alterative doses, under the most

guarded and judicious mode of administering that medicine. In such cases, I place much more reliance upon the administration of hydriodate of potash, in conjunction with sarsaparilla. When the presence of nodes indicates the utility of mercury, I restrain myself from its exhibition should rupia also be present, from experience of its injurious effects on the general disease, under this form of eruption. And even when extensive ulceration of the fauces, engaging the velum, tonsils, and entire pharynx, seems to threaten the life of the patient, I would try every method likely to succeed, before I should have recourse even to mercurial fumigations, for fear of mercurializing the *entire system*, although well aware of the benefit often arising from their use as a *local remedy*. The local remedies upon which I chiefly rely in checking the progress of this dangerous ulceration, are the application of a strong mineral acid—the white muriate of antimony—a saturated solution of the oxymuriate of mercury in alcohol—or of nitrate of silver in distilled water. While the constitutional remedies, upon which I most depend, are hydriodate of potash, sarsaparilla, and opium in sufficient doses to relieve irritation.

I have found mercury, in every stage of the phagedenic venereal disease, to be a most deceitful remedy; for, although symptoms may amend for a brief period under its use, and flatter both patient and practitioner that a speedy cure is at hand, yet, almost to a certainty, new symptoms will arise to disappoint those sanguine expectations. If mercury is at all admissible for this form of venereal disease, it is, as I before observed, when the malady is obviously on the decline, and that the eruption has assumed the appearance of scaly tubercles or blotches. This observation equally applies to the pustular form of venereal disease. It is not my wish to enter farther here into the consideration of the different forms of venereal diseases, as the object of this paper is merely to point out, from my own experience, where mercury may be found useful, and where injurious.

6th. For the true Hunterian chancre, with hardened edge

and base, and for the scaly eruption, either lepra or psoriasis which attends it, as well as the deep excavated ulcer of the tonsils, nodes, and other symptoms belonging to this form of disease, mercury, in full doses, may be esteemed a certain and expeditious remedy; and the reason of the necessity of exhibiting mercury seems to be, that both in its primary and secondary symptoms there is but little or no accompanying inflammation or fever as in the other forms of those maladies. Hence, perhaps, the utility of raising artificially a fever in the system, to overcome the morbid effects of the poison. I have no doubt, however, but that even this form of venereal may yield to other remedies, or even to the unassisted powers of the constitution. But from the few instances I have seen treated on the antiphlogistic plan without mercury, so long a period elapsed before recovery took place that it is not likely this remedy will ever be generally omitted in its treatment. It is true, that some years since, on visiting the great hospital at Montpellier, the late much lamented M. Delpech pointed out to me some cases of chancre successfully treated by destroying the callous base of the ulcer with caustic, a practice which that celebrated surgeon told me he had for some years adopted with the most satisfactory results.

Having now briefly stated the circumstances under which I have been in the habit of employing mercury, I find it necessary, in explanation, to allude to my classification of venereal diseases, grounded on the nature of the eruption, as it is denied by many that there exists any connexion between a particular form of primary and a particular form of secondary symptoms. This surprises me not a little, as I have been in the habit of meeting instances of this connexion in hospital daily from year to year, and pointing it out to the pupils, particularly that of the papular and phagedenic eruptions, with their corresponding primary symptoms. The papular and phagedenic forms are the two great families of venereal diseases, which at present are most generally to be met with; the one admitting of a certain and easy

cure ; the other, on the contrary, is too often an unmanageable, obstinate, and destructive disease, particularly in such cases as have been subjected to mercurial treatment. Now, if we make allowances for difference in age and constitution, for neglect, intemperance, mode of living, exposure to cold, the various causes of irritation, and the premature or unguarded use of mercury, we shall find that there are but few instances in which we may not be able to decide on the nature of the primary ulcer after the antiphlogistic regimen, rest, and appropriate local treatment, calculated to relieve the effects of irritation, have been adopted.

And when we consider the influence of these various causes on the most simple ulcers, we cannot but acknowledge the great variety of appearances which they must necessarily occasion,—so numerous, that it would be utterly impossible to describe or delineate all the shades of difference to be found in venereal ulcers, as some have vainly attempted. But it is contrary to all we know of the laws of morbid poisons to suppose, that an ulcer irritated or affected by any of the causes above-mentioned, should, instead of producing the mild, papular eruption which ends in desquamation, excite that of rupia, which spreads into extensive phagedenic ulceration, with those other baneful and destructive concomitants of this form of venereal disease.

I have arranged primary venereal ulcers into four classes, according to the eruptions: papulæ, pustulæ, rupiæ, and lepra or psoriaris, which I have traced to them. There may be others, for I never pretended to have done more than to have made some advances in the natural history of venereal diseases, the importance of which I leave to the unprejudiced to decide.

Venereal eruptions, like primary ulcers, must be more or less modified by the same circumstances which influence the appearance of the latter ; so that it may often be difficult to decide upon their true character ; and I admit that we often meet with a copper-coloured, mottled appearance of the skin, neither

elevated or scaly, but decidedly venereal, which I have not been able to trace to any particular form of ulceration.

From the intervention, also, of the influential circumstances above noticed, a venereal eruption may be so affected or modified, as not to indicate distinctly the particular form to which it appertains; yet I shall venture to say, that the *tendency* to one or other of these forms will always be sufficiently obvious, for all practical purposes to mark the true nature of the eruption, and consequently of the disease under consideration.

It is often objected against my classification, that a patient will sometimes exhibit on him at the same time each of these forms of eruption. This objection I have answered elsewhere, observing that papulæ have constantly acuminate heads containing matter, which some might call pustules: but the true diagnostic distinction between venereal papulæ and pustules is, that the former end in desquamation, the latter in superficial ulcers.

If a case presented itself in which pustules that terminated in ulcers were mixed with papulæ, I would say the patient was afflicted with the pustular eruption: always designating it by the term which signified the most severe and dangerous spots which appear on the patient. For in the pustular eruption we often meet with papulæ, and in the phagedenic eruption we often see the rupia, which marks the disease, intermixed with both papulæ and pustules; but the first, in my judgment, indicates, in each form, the true nature of the eruption.

In the same manner, in small-pox, we often observe papulæ intermixed with the pustules, which latter constitute the characteristic signs of the disease; for no person thinks of calling small-pox a papular, but a pustular, eruption.

Besides all venereal eruptions, as I have already observed, have a tendency, as the disease becomes exhausted, to exhibit the scaly appearance, and therefore the necessity of attending to their peculiar characters when they first occur. A consideration of these circumstances will sufficiently evince, how easy

it is for *prejudiced* or superficial observers to overlook, either *intentionally* or unintentionally, the connexion which exists between particular forms of primary, with particular forms of secondary eruptions. If the distinctive characters of venereal symptoms, both primary and secondary, did not carry with them a practical bearing of the utmost importance, I should not trouble my reader by calling his attention to them ; but I have been for years in the habit of pointing them out to the pupils at the bed-side of the patients, and showing the great use in practice, which a thorough acquaintance with them affords. However, as long as surgeons are impressed with the belief in the doctrine of the unity of the venereal poison, and that there is only one cure for it—mercury, such distinctions, in a practical point of view, are totally useless, and in their estimation only calculated to gratify an innocent curiosity.

It surprises me, at least, not a little, to find the connexion which exists between the primary phagedenic ulcer, and the secondary symptoms, rupia and tubercles, spreading into deep ulcers—extensive ulcerations of the fauces—foul ulceration of the mucous membrane of the nose, with caries of its bones, and the other alarming and disgusting chain of symptoms which I have elsewhere described as appertaining to the phagedenic venereal disease—it surprises me, I say, that this connexion has not been more universally acknowledged, as well as the injurious effects of mercury upon it. For I may safely say, that in the Richmond Surgical Hospital we are never without examples of these facts, which are therefore as familiar to the pupils of that institution as any other surgical phenomena.

Happening to cast my eye over the last Number of the Medical Gazette, (No. 16, for January 13th), I found a clinical lecture of my friend Mr. Laurence on Venereal Diseases. The very first case he lectures on is as strong an instance, detailing the presence of the primary ulcer, and the constitutional symptoms which it occasions, together with the ill effects of mercury upon both, as I could possibly wish to adduce from the re-

cords of my own experience ; and therefore the statement coming from one of the highest authorities in the profession, must carry with it great weight, particularly as the testimony comes from one more opposed than I imagined to the views I have given respecting this form of venereal disease, and of the ill effects of mercury upon it.

The circumstances of the case I shall endeavour to curtail, are briefly as follows: William Smith was admitted into St. Bartholomew's Hospital on the 19th of May, 1836, with an extensive *phagedenic ulcer* of the prepuce, extending to and engaging a portion of the glans. A few large brownish red tubercles were on the face and forehead. He was directed to take two grains of calomel, with one-third of a grain of opium, three times daily, and black wash was applied to the ulcer. Under this treatment, it seems, the greater part of the ulcer healed, except on the dorsum penis, to which it had extended, and burrowed to so great an extent as to render it necessary, on the 13th of June, to lay open the undermined skin by a free incision. The ulcer was afterwards dressed successively with black wash and balsam of Peru. The sulphate of quinine and compound decoction of sarsaparilla were now administered, and it seems, under this treatment, the patient recovered, and was discharged the hospital, apparently in excellent health. However, he returned again on the 10th September following, with a relapse of the ulceration of the penis. Mr. Lloyd, in the absence of Mr. Laurence, applied nitric acid to it, and after the separation of the slough, a clean granulating sore presented itself. He was again discharged apparently well in October. However, "on the 6th of January, 1837," observes the lecturer, "he was again admitted with phagedenic ulceration of the fauces and face, and inflammation of the periosteum. The velum palati, uvula, and tonsils, the upper and back part of the pharynx, were occupied by an irregular ulceration with a lardaceous surface, ragged edge, and bright red margin : deglutition was performed with difficulty and with great pain. There

were three or four circular phagedenic sores on the face; there was a soft, fluctuating swelling over the left superciliary ridge, and another on the os malæ of the same side. The carpal extremity of the left ulna presented a large painful swelling of the periosteum. All these affections were of painful character; they had interrupted rest, and impaired appetite, causing emaciation and great weakness. These evils were aggravated by great alarm respecting the nature and progress of the disease, and depression of spirits. I found, on inquiry, that he had never been affected with syphilis before; that he had been of regular habits; and that he had been particularly careful of himself since his first admission into the hospital. The painful nature of the symptoms, and the enfeebled condition of the patient, required narcotics, and strengthening means, both medical and dietetic, and presented a strong contra-indication to the general use of mercury, although the influence of that remedy was required to check the progress of disease, particularly in the throat. He was ordered to fumigate the throat with cinnabar every night and morning; to take the concentrated compound decoction of sarsaparilla three times a day, and 5 gr. of the pil. saponis c. opio at night; and to feed on milk and broth, or on meat, according to his powers of swallowing. On the 9th, eight ounces of port wine ordered for him daily, and the dose of the pil. saponis c. opio was increased to $7\frac{1}{2}$ gr. On the 24th, the fumigation was continued once daily, the mouth having become sore, and linctus was ordered for a cough. Under this treatment, the local symptoms and the health improved rapidly; the ulceration of the throat lost its phagedenic character, and soon healed; the ulcers of the face cicatrized; the swelling of the ulna disappeared, and those of the frontal bone and os mala broke and discharged, the openings subsequently scabbing over. With the abatement of pain the rest and appetite returned; the flesh and strength were restored; and the patient left the hospital in the middle of February, not only free from disease, but stout and in excellent health."

It seems that this excellent health did not long continue, he was again re-admitted and put into the *Lazarus Ward*, a significant domicile for patients worn to a skeleton, in hopes, I presume, of a resurrection that was little to be expected. "His symptoms were," continues the lecturer, "disease of the nose, ulceration of the throat, enlargement and induration of the right testicle. He came in on the 23rd of this month, (June.) There is a large ulcerated opening in the septum narium, forming a free communication between the two nostrils, both of which are in great measure blocked up with bloody scabs and stinking matter. A most offensive foetor is diffused to some distance round the patient. The bridge of the nose has partially sunk, and has become turned to one side. The integuments are swelled; of dull red colour; hot, and painful. There can be no doubt that the bone is diseased, from the yielding of the septum, the offensive stench, and the inflamed state of the external coverings. The affection of the mucous membrane is probably secondary; the seat of disease in the pharynx is its upper and back part, so high up that the lower portion only of the ulcer comes into view; it has the same phagedenic character as on the former occasion. This position of the ulcer in the pharynx, if not peculiar to phagedenic syphilis, is very common in that form of the disease; the testicle is moderately enlarged, hard, somewhat irregular and knotted, painful on pressure; the scrotum is red, and rather warm.

"The remedies ordered were: solution of corrosive sublimate (half a grain to the ounce) in lime water, to the nose, previously clearing it of scabs and matter by means of tepid water; five grains of hydriodate of potash in an ounce and a half of compound decoction of sarsaparilla, three times a day; mercurial liniment to be rubbed on the testicle. The patient has now been in the hospital a week under this treatment, and already feels much better.

"Smith remained in the hospital four or five weeks on the

last occasion. Before he went out, the whole septum narium came away in several pieces. The discharge ceased, and the nostrils became clear, but the deformity of the nose was increased ; the throat soon got well, and the swelling of the testis subsided. He showed himself at the hospital from time to time during the autumn, continuing free from disease and in excellent health. It may be concluded that he continues well, as he promised to return in the event of any relapse."

Mr. Laurence will take in good part any observations I have made or shall make on this case, because he will see they are not intended to affect that high estimation in which he is so justly held by the public and the profession, but because the treatment he pursued is that which perhaps the majority of hospital surgeons in the British Islands would have advised under similar circumstances ; and therefore it is against the established practice, and not against him, that my animadversions are directed.

This case, as well as a multitude of others of the same character, convince me that mercury is, for this form of disease, a most fallacious and deceitful remedy ; that it sometimes causes the ulcers to extend ; at others it flatters with an early amendment, which soon ends in disappointment, by the extension of the old symptoms, or the supervention of new ones ; and in all instances it renders the general disease intractable ; nay, I will go so far as to say, incurable ; and notwithstanding that this patient left the hospital apparently in excellent health, under the use of the very medicine I would have prescribed myself, I shall venture to say, that in consequence of the mercurial process to which he had been subjected at an early period of the disease, that he is not yet free from the malady.

It will naturally be inquired, how would I have treated the case. Without presuming to dictate to others, I shall briefly state, that were a patient to come under my care with a similar phagedenic, primary ulcer, and the constitutional symptoms

described ; I should, in the first instance, destroy the surface of the ulcer, by a strong mineral acid, (as was afterwards on a second admission successfully done by Mr. Lloyd,) or by the white muriate of antimony ; I should at the same time endeavour to relieve pain and irritation by repeated doses of opium, so as to keep the system under the narcotic influence of that most useful of all medicines. Afterwards, when pain and irritation were relieved, I should exhibit, with a view to the secondary symptoms, hydriodate of potash in combination with sarsaparilla, in such doses as the stomach was capable of bearing without inconvenience. The compound decoction, or the compound infusion in lime water of the latter, are the preparations I prefer. Under this simple plan of treatment, I have succeeded in bringing numerous cases, similar to that described by Mr. Laurence, to a happy termination. But if mercury had been previously exhibited, as in the present instance, the case must be esteemed much more complicated and unmanageable, than if it had not.

There are two modes of assailing an individual who imagines he has made some improvement in any art or science. 1st. To shew that the supposed improvement is no improvement at all ; and 2ndly, that the improvement had been previously well known. I have been assailed in both these ways. I shall not now, however, stop to inquire whether the introduction of the non-mercurial treatment has, or has not, been of advantage ; but take the opportunity of stating my claims to its early promulgation, as I find that the merit (and it is even acknowledged by the most inveterate mercurialist, that it has been of some advantage) is attributed to others, who certainly have not any claims which can compete with mine.

In 1810 I was appointed one of the surgeons of the Westmorland Lock Hospital of Dublin, containing at that period about three hundred venereal patients. About this time Mr.

Abernethy's work on Pseudo-Syphilitic Diseases made its appearance ; and the cases adduced by that original and celebrated man made the strongest impression on my mind. But contrary to his assertion, that the symptoms of the resembling diseases could not be distinguished by their appearance from those of syphilis, I was convinced in my mind, that if differences existed in nature, they would be manifested by a difference in the characters of the symptoms ; and therefore determined upon bringing this view to the test of experiment, by making use of the extensive opportunities I possessed.

I therefore, soon after my appointment, commenced an investigation by observing accurately the various appearances and characteristic distinctions of venereal complaints, both primary and secondary, and by treating all those cases *without* mercury which did not correspond with Hunter's description of true syphilis. The result of the investigation exceeded my warmest expectation. It proved not only that the received dogma of that day, that venereal diseases progressed without the intervention of mercury until they destroyed the patient, was without foundation ; but it also demonstrated, that the great majority of those complaints could be perfectly cured in a much shorter period than is usually effected by the intervention of mercury.

In 1813 I delivered a course of lectures at the Lock Hospital, on venereal diseases, to a very numerous class, not only of pupils, but of practitioners ; to whom I communicated the facts developed by my investigation, at that time scarcely credited, on account of their novelty and opposition to the received doctrines which governed the practice of medical men. The first lecture of this course, according to a printed syllabus which lies before me, was delivered on the 29th of March, 1813 ; just a quarter of a century ago.

Early in 1814 was published the first edition in 4to of my work on Venereal Diseases, containing plates of the four great varieties of venereal eruptions.

In October, 1815, I published a paper in the Medical and

Physical Journal (No 200,) containing a statement of seventy cases of venereal disease treated without mercury, the majority of which were cured (as was then thought) in an incredibly short period ; and their authenticity would have been doubted, had they not occurred in two public hospitals, the Lock and the Richmond, under the observation of numbers of professional men. In a note at the conclusion of the paper, the Editor had the kindness to make the following flattering observation.

“ The great mass of evidence contained in seventy well authenticated cases render unnecessary any apology for the length of Mr. Carmichael’s paper, and we must impress our readers with the same sense of gratitude to the author as we have felt. Henceforth we hope to hear no more of the impossibility of finding discriminating characters in cases where the question is no less than the exhibition of a remedy which confounds all character, and has proved destructive in many complaints which would have healed spontaneously or yielded to mild remedies. We hope also those much too general terms of *pseudo-syphilis* and *syphiloides*, which remind us of the early and more imperfect state of botany, will gradually fall into disuse, and evince an improvement in medicine by giving way to descriptive names.”

In 1818 I published a small work entitled “ Observations on the Symptoms and Specific Distinctions of Venereal Diseases interspersed with hints for the more effectual prosecution of the present inquiry into the uses and abuses of mercury in their treatment,” which I felt much pleasure in dedicating to Sir James M’Grigor, Director-general of Military Hospitals. This mark of attention I conceived due to Sir James, in consequence of that exemplary and excellent officer having recommended my system of treating venereal diseases to the consideration of the surgeons of the British army, although at that period I had not the pleasure of being personally known to him.

Shortly after this last work was published, I received a very flattering letter from Sir James M’Grigor, stating that he “ had

transmitted a copy of it to every regiment in his Majesty's service, in every part of the globe where British troops were stationed."

In 1825 a second edition of my first work was published, with considerable additions. I have taken the liberty of obtruding these dates upon my professional brethren, because, in various publications, reviews, and published lectures, I find the merit of commencing the anti-mercurial investigation has been attributed to the late Mr. Rose, Surgeon to St. James' Infirmary, and to the Coldstream Regiment of Guards. His communication on the subject is to be found in the 8th vol. of the *Medico-Chirurgical Transactions*, and was read on the 24th of June, 1817.

A comparison of the date of his paper with my first publication of 1814, on the subject, without taking into consideration my lectures at the Lock Hospital, in 1813, needs no comment in order to settle the question of priority. Indeed, he could have no intention himself of laying any claim to it, as my publication is frequently alluded to, both in his communication to the society, and that of Mr. Guthrie, which was read on the same night, and published in the same volume. It might, therefore, as well have been attributed to the latter gentleman, but Mr. Guthrie having survived his competitor, the time has not yet arrived for conferring on him a similar honour.

No doubt, from the earliest period after the introduction of mercury for the cure of venereal complaints, there arose, from time to time, a few clear-sighted individuals, who, seeing evidently the mischiefs which its indiscriminate use occasioned, doubted its claim to the character of a specific, and without deceiving themselves and others by naming the symptoms which did not yield to its influence, either syphiloidal or mercurial, they had the boldness to treat venereal cases without that mineral. But I do not know of any before myself who tried the anti-mercurial treatment on an extensive scale, such as I had the

opportunity of doing in two large public hospitals, and afterwards published the result of these experiments.

Independent of the trials the anti-mercurial treatment has had in those islands, and in the British army, it has of late years met with the most extensive experience in France, Germany, and Sweden, as may be learned by the publications of Desruelles, Cullerier, and Divergie in France, by those of Oppenheim, Fricke, and Dietrich in Germany, and by that of the Royal Council of Health in Sweden, as well as by many other indisputable authorities, from which sources we have the most authentic information of the recovery of thousands—nay, tens of thousands of venereal patients, in the course of a very few years, without the exhibition of a single grain of the specific. Therefore, let us hear no more of the necessity of subjecting every venereal patient to a mercurial course, the indiscriminate adoption of which practice, I have no hesitation in asserting, has sent ten times greater numbers to an untimely grave than the disease it was intended to cure.

As I have been called on for a third edition of my work on venereal diseases, I shall postpone for the present the consideration of these important documents, and the inductions which the anti-mercurial treatment on the Continent affords, but shall give below those of M. Desruelles,* marking in italics those which

* “ 1^o Qu'on peut guérir les maladies vénériennes, en employant seulement un traitement simple, sans mercure, et sans autres moyens thérapeutiques que les adoucissans et les antiphlogistiques.

“ 2^o *Que le régime végétal et adoucissant doit être la base de tout traitement, avec ou sans mercure.*

“ 3^o Que les mercuriaux ou autres moyens, qui aut été préconisés contre les affections syphilitiques, ne doivent plus aujourd'hui être considérés comme des spécifiques, et que leur emploi ne doit être envisagé que comme déterminant une révulsion nécessaire pour obtenir la modification curative que le traitement simple ne saurait produire, dans tous les cas et chez tous les individus.

“ 4^o *Que, si cela est possible, il faut guérir localement, et dans le plus bref délai, les maladies primitives ;*

are particularly deserving of attention, and shall conclude by expressing my regret that so many of the profession should still continue inattentive to the advantages which a close observation of the characters of both primary and secondary symptoms afford in pointing out the most appropriate mode of treatment, and affording, also, an excellent foundation on which to form a just prognosis. I have also to regret that the profession should so gene-

“ 5° Que traitées ainsi, soit par la cautérisation, les astringens, ou les antiphlogistiques actifs, elles donnent moins de chance à la production des maladies consécutives ;

“ 6° Qu’il ne faut renoncer à aucun des moyens qui entrent dans la thérapeutique de l’ancienne méthode ; *mais qu’il faut les mettre en usage dans certains cas et non dans tous.*

“ 7° Que, quelles que soient les affections contre lesquelles on les emploie, il faut y associer les antiphlogistiques, surveiller leur action sur l’organisme, et les donner à dose modérée et convenable pour éviter les fâcheux accidens qu’ils produisent quelquefois.

“ 8° Que, quel que soit le traitement employé contre les maladies vénériennes primitives, *on ne peut jamais être assuré qu’il ne surviendra pas des affections consécutives*, contre lesquelles il sera nécessaire de déployer tous les ressorts d’une thérapeutique raisonnée ; *mais que d’après les nouvelles expérimentations, le traitement simple a des recidives moins nombreuses, moins étendues et moins profondes, que le traitement mercuriel.*

“ 9° Que le traitement simple, qui laisse subsister moins longtems les maladies primitives, et met l’organisme dans des conditions peu favorables au développement de l’irritation, est celui que l’on doit admettre d’une manière générale, parce qu’il fait courir au malade des chances moins nombreuses de récidive ; *et que ces récidives, étant moins graves, guérissent plus facilement que celles qui résultent du traitement mercuriel.*

“ 10° Et enfin que le mercure et les autres moyens révulsifs doivent être réservés pour les affections consécutives qui ne pourront être vaincues par le traitement simple.

“ En établissant ces préceptes et ceux que nous ferons connaître à l’article *traitement*, nous ne prétendons pas rejeter la méthode mercurielle ; nous pensons, au contraire, qu’elle peut être utile, dans certains cas, précisés avec quelque certitude par l’observation, et qu’alors l’association du traitement simple et des mercuriaux ou autres moyens stimulans, aura de grands avantages entre les mains d’habiles praticiens.” — DESRUELLES des *Maladies Veneriennes*, p. 146.

rally arrange themselves into two opposite factions—the mercurialists, and the anti-mercurialists. The one party subjecting all cases, indiscriminately, to mercury, while the other as obstinately persists in not administering it under any circumstances.

To the first I would say, attend to the distinctive characters and stages of the disease under consideration, and learn to discriminate one form of venereal from another, as each form requires an appropriate mode of treatment ; but, above all things, do not, in the present improved state of medical science, lay yourself open to the charge of empiricism, by administering mercury as a specific or nostrum. And to the anti-mercurialists I would say, do not refuse the aid of so active a medicine as mercury, the powers of which are well known and understood in remedying various morbid conditions of the frame, but give it either in full or alterative doses where you think those powers can be called into active operation for the benefit of your patient, under the guidance of those sound principles of pathology and therapeutics, which would guide you in its exhibitions for other diseases. By following this advice, those belligerents may at length be reconciled, and subscribe to the truth of the old adage, that *media via est tutissima*.

6th February, 1838.

P. S.—After this sheet was put to press, I found, in the last Number of the *Lancet*, (XX., 10th February, 1838), a case of phagedenic ulcer of the penis, detailed by Dr. BurrIDGE, Physician to the Taunton and Somerset Hospital, which is, if possible, a more striking example of the ill effects of mercury upon this form of venereal disease than that related by Mr. Laurence. I might, from my own experience, detail hundreds of analogous instances, but I prefer giving that of others. Dr. BurrIDGE's case is as follows:—

“A young man, æt. 19, having contracted a chancre, *was severely mercurialized by a quack*. When he first came under my notice, *he had a phagedenic ulcer* behind the corona glandis.

Bark and opium were freely administered, local applications of various kinds employed, including nitrate of silver and nitric acid, but without success; the glans separated. Buboes occupied his groins, which assumed a phagedenic character on being laid open, giving rise, at length, to a false aneurism of the femoral artery, for which it became necessary to tie the external iliac. This operation was succeeded by the usual symptoms of phlegmasia dolens. During this attack, inflammation of the superficial veins of the limb manifested itself; these veins suppurated, still bearing the phagedenic character; and the cartilages of the knee-joint were exposed. A sphacelus appeared in the front of the tibia; gangrene extended over the internal malleolus and anterior part of the foot, though apparently healthy granulations were thrown out from above downwards; the tibia and fibula were exposed, and their outer laminæ necrosed. It was about four months from the time when the external iliac was secured, that the leg was removed above the knee, and in about a week the young man sunk under a pulmonary affection. No tubercle was found in the lungs, or mesentery, though he was of strumous habit. Throughout his long and melancholy sufferings, he received the constant and anxious attendance of more than one surgeon of first rate eminence."

Dr. BurrIDGE, after this sad detail, need scarcely have demanded, "Is it too much to affirm, that this unhappy youth was the victim of mercury misapplied?" And, in another place, he observes: "The indications for the employment of mercury are still amongst the *desideratissima* of our science, and still the wonder grows that no man of competent talent and experience applies himself to the task of elucidating so interesting a point, and the rather as the frequency of these cases adds not a little to our daily embarrassment and annoyance."

Perhaps Dr. BurrIDGE and the profession may find in this paper some striking facts and useful views tending to elucidate this interesting and most important inquiry, *where mercury is admissible, and where not admissible, in venereal diseases.*

BIBLIOGRAPHIC NOTICES.

Gleanings from Foreign Medical Journals. By Dr. BIGGER.

Gout in the Penis.

DR. WEISE of Fraustadt, gives the case of a shoemaker, æt. 45, who was regularly attacked by gout in the feet every year, which usually continued many weeks. It came this year also, and had been present three weeks, when it suddenly left the feet and attacked the virile member. The pain was so violent that the penis remained in constant erection, and the suffering of a cutting character became most extreme in the afternoon, and continued so till midnight, often forcing the patient to cry out violently; this pain sometimes was felt at the glans, sometimes at the root, and sometimes in the middle of the penis.

This patient never had had any disease of the genital organs. His urine was generally natural, although the discharge of it was slow, its colour clear and yellow, and only very seldom had small particles of gravel, of reddish yellow colour been perceived in it. At times, when the employment of internal and external remedies had diminished the pain, still the symptoms returned in the evening with even greater violence. After three weeks this state of things ceased, and the affection took possession of the right foot and left elbow joint. Continued perspiration, and turbid urine, announced the crisis; all the symptoms after fifteen days disappeared, the patient felt himself perfectly well, and has since remained so.

Epilepsy cured by Indigo.

A boy, æt. 10, had in the year of 1833 an attack of epilepsy, in consequence of cold and a fright which he had received.

Many remedies had been employed without the least advantage, when Dr. Hohnhorst of Frankfort was called in, who gave the oxyd. zinci, in increasing doses, till the dose amounted to eight grains; this treatment acted so beneficially, that the epileptic paroxysms disappeared until the winter of 1834-5.

In 1835 the boy accidentally broke the ice on a frozen ditch, and fell into the water, when the epilepsy set in again, but the fits were slight, and generally came on in the night or towards morning.

In the June of the same year, the lad fell in the street with his head on the pavement, for which the surgeon had to be sent for again. He found the lad senseless, with symptoms of severe injury of the head, and from time to time repeated epileptic attacks. On the right side of his forehead was a swelling of remarkable size, which extended from the brow to the coronal suture, and a wound of the skin the whole length of it.

On the 26th of June an incision was made through this swelling, and three or four ounces of coagulated blood which had been situated between the bone and epicranium, given exit to. The bone was uninjured. Twenty-four hours later violent lymphatic inflammation set in, which however moderated after a few days, and then ceased.

On the 28th, he recovered the use of his senses, and on the following day his speech, which, however, still was indistinct, and he found difficulty in pronouncing some words. The pus from the wound became healthy, and the wound healed. The epileptic convulsions came on both in the day and night, and so numerous were the fits, that two often occurred in the same hour, and so many as twenty-eight fits were counted in twenty-four hours. Under these circumstances Dr. Hohnhorst determined to administer indigo on the plan proposed by Ideler. He commenced on the 4th of July, giving two drachms in the twenty-four hours, and increasing the quantity given every three days by half a drachm. On the 4th of July the first decrease of the epileptic seizures was observed, inasmuch as only twenty fits occurred in twenty-four hours; they decreased, both in number and in duration, gradually and regularly under the use of the indigo, so that on the 30th of August there was only one paroxysm, and it was the last.

On the 20th of August so much as an ounce and a half of the remedy was given in twenty-four hours: this was diminished gradually as it had been increased, until only two drachms were taken in the twenty-four hours. By the end of October only one drachm was administered, and then the remedy was

entirely discontinued. — *From the Beiträge zum Sanitäts-Berichte des Frankfurter Regierung's Bezirks.*

Partus per Anum.

Dr. Mekeln, of Kettwig, was called to a female on the 1st of January, who had given birth to a strong and living infant through the anus, two hours before his arrival. The wound in the under part of the vagina as well as that in the rectum was of great size. The perinæum, from the aperture of the anus to the vagina, was two-thirds torn, and very painful.

After three days both the urine and fæces passed by their ordinary channels.

On the fourth day suppuration occurred ; the wounds healed, and the woman in due course recovered her strength. Dr. Mekeln declares that he could discover no defect in the organization of the parts. The midwife states that at her arrival, she found the head of the child in the rectum.—*From the General Sanitäts-Berichte des Königlichen Medicinal-Collegium's.*

Extraordinary Size of an Infant at Birth.

Dr. Thümen, of Penzlaw, gives the case of a woman for whom he had brought nine children into the world by the aid of the forceps. She was strong and robust. Whilst pregnant of her tenth child she experienced much pain in the inguinal region, with uneasiness and loss of power in the whole right leg during labour. By great efforts the head of the child was freed, the shoulders remained behind, and resort was obliged to be had to the blunt hook to liberate them. Then it required still greater force to extricate the pelvis of the child. It was a boy, and weighed twenty pounds.—*From the Beitragen zum Sanitäts-Berichte des Potsdamer Regierungs'-Bezirks.*

Curative Effects of Nitrate of Silver in Nervous Diseases of the Stomach.

Dr. Steinitz, of Greiffenberg, states, that from his observations on many cases he is enabled to give evidence of the great efficacy of the nit. arg. in nervous diseases of the stomach. This remedy was first introduced to public notice by Autenrieth* and Ruff.†

* Froriep's Notizen, Bd. 26, No. 26.

† Heidelb. Annalen, 1836, Bd. 2, Hft. 1.

Dr. S. employed it in cases in which the nit. bismuthi, castor, zinci hydrocyan., and morphine had been used endermically, as well as many other means, without any good effect, and that the results were extraordinary. The disease was treated generally in delicate nervous constitutions in men, but principally in females, on whom the remedy did not produce any injurious sympathetic effects, and for the most part, the dose, as given below, when once employed, or after an interval a double portion, was found to allay the disease, without any relapse having occurred.

It is necessary, before employing this medicine, that any, even the least, inflammation of the stomach should be perfectly cured. The cases for its employment are those in which the patient, after having suffered from depression of spirits, or from prolonged night watching, feels from time to time an unendurable sensation of weight and uneasiness in the region of the stomach, which is rather alleviated than increased by pressure of the hand on the part, so much so, that the patient is observed to press himself with his closed hand, or to cause some one else to exercise pressure on the pit of the stomach, in order to diminish the uneasiness; with this there is an unpleasant feeling of emptiness, a desire for stimulating food and drink, insatiable hunger, general depression of spirits, and often evacuations of limpid urine. Dr. S. states that he has not yet had opportunity of employing nit. arg. in those cases of continued vomiting which appear consequent on a state of incipient disorganization of the stomach; however, Dr. Ruff has given it successfully in such cases.

The formula for this remedy is as follows:

℞ Argenti Nitrici Crystallizati, gn. v.
Solve in Aquæ Distillatæ q. s.; adde
Extracti Taraxici,
Pulveris Radicis Liquiritiæ ā ā ʒss.
M. Ut fiat massa, in pilulas xx. dividenda.

One or two of these pills to be taken early in the morning, and again in the evening with mucilaginous drinks.

Dr. S. remarks, that it is principally metallic means which produce the most beneficial effects in neuralgies and diseases of the nerves. However, it is necessary to know the special agency of each particular preparation, in order to apply it properly to that affection of the nerves for which it may be appropriate. That with regard to some particular metals, as for example, the sulphate of copper, physicians are probably in the right track; its beneficial effects in croup depend both on its action as an emetic, and on its specific action on the nerves

of the larynx and trachea. That in nervous attacks of croup, which occur very frequently with complications in this organ so abundantly provided with nerves, he had found, by most attentive observation, that the ammoniated sulphate of copper acted specifically on the nervous affection, but had no influence over the already formed genuine inflammatory croup. An excellent essay has been written on this subject by Dr. Köchlin of Zurich, in his work on the Effects of Metals on the Human Constitution, published in 1837.—*Medicinische Zeitung*, No. 40, 1837.

Communication of the Poison of Rabies from one human Being to another. By Dr. HAXTHAUSEN, District Physician at Neisse.

A servant received a slight wound from a young shepherd's dog in the right hand, and eight weeks afterwards became affected with hydrophobia, although he placed the most implicit confidence in a quack remedy, employed by the peasants in his neighbourhood, which was applied immediately after the accident. He had felt sharp pain proceeding from the cicatrix of the wound up the arm to his breast, many days before the appearance of the disease. No vesicles appeared on the tongue, but there was swelling of the glands, and although he was in full possession of his senses, yet he had a strange, unaccountable feeling at intervals. The longer those periods lasted, so much the longer and more stormy were the hydrophobic attacks.

In one of these paroxysms, rendered remarkable by his spitting, biting, and scratching, he suddenly broke away from his attendants, and scratched the face of a servant girl who was present. This girl, a strong-minded, phlegmatic person, about 20 years of age, immediately washed her face, and regarding the accident as an indifferent occurrence, thought no more about it, until the night of the 16th of May, when she found herself indisposed. Her temperament had become hasty and exalted, and quite different from what it had been formerly. Her eye became bright, and at times sharply fixed; her thoughts, contrary to her usual wont, seemed busy; sometimes she would appear to listen, then feel astonishment. These symptoms led to apprehensions of a serious illness, but before this had occurred the girl was brought home to her parents in a neighbouring province of Austria. From thence we received information that hydrophobia had set in with all its symptoms. She felt shuddering and spasmodic constriction of the throat at the sight of fluids, with sensation of choking and twitching

motions of the whole body. The attacks came on periodically, and were particularly characterized by wild shrieking, and a wish to bite and spit, as well as to destroy every thing. The latest news we received was that the girl was still alive, and that the fits, with ever increasing periods of intermission, had become gradually slighter, and assumed in a remarkable manner the nature of epileptic convulsions, although previously she never had had an attack of this kind. These paroxysms occurred very seldom in the latter periods and were slight, yet they were principally excited by some remembrance of the man who had died. The medical treatment, after the abstraction of blood, had been of a simple nature.—*From the Medicinische Zeitung.*

External Application of the Acid of Haller.* By Dr. BRACH of Neustadt, District of Cologne.

Useful as is the employment of the mixture of sulphuric acid internally in some diseases, its external application in other diseases is equally beneficial, nay, in some specific. Without attempting to explain the mode of its action, I must content myself by describing those cases in which I have employed it with the greatest advantage.

1st. In painful rheumatic and gouty affections, which are free from fever, and without any important inflammation, I cause the acid of Haller to be rubbed two or three times daily on the most painful parts, whether it may be on the head, the arm, the leg, where a single limb only is affected, and where the attacks come on more or less periodically in pain of the hip and loins, or of the face. Frequently the most violent pain ceases after two or three rubbings, often after the first, and in most cases the object to be desired is perfectly attained, though not in all. Cases in which the hitherto known remedies, viz. the application of narcotic and numbing medicines externally, and internally, even opium in large doses and leeches have all failed, yet in those cases the rubbing in of the acid of Haller will assist materially in expelling all the pains. I could relate many cases in which it has been of the greatest service.

2nd. In dissipating indolent swellings, lymphatic obstruction, induration of glands, local aqueous collections, dropsy of the serous membranes, and of the joints, &c. I know no more appropriate means than this.

* According to Arnemon's *Materia Medica*, the acid, elixer of Haller, consists of equal parts of concentrated sulphuric acid and of rectified spirits of wine.

A year and a half ago I had a patient, a man of 50 years of age, under treatment, who suffered from swelling of the knee joint, which had all the symptoms of dropsy of the joint, the swelling was largest on the anterior and side parts, and seemed extending. On stretching out the limb the fluctuation was evident; when it was bent, the swelling was greatest on the sides, and the very moveable patella was pushed much forward. Besides there was some slight pain on pressure on the joint, but not much. There was no heat beyond what was natural. It was only on a change of weather that shooting pains occurred, and this continued for two months. Besides, on the inner side of the joint there was a round, elastic, defined, spongy feeling, immoveable tumour of the size of a small hen egg, which I considered to be a dropsy of the bursa mucosa.

I caused the whole joint to be rubbed three times a day with the acidum Halleri. After three days the affection had entirely disappeared, the defined swelling was no longer to be felt, and there was no pain. Three months later the same man came to me again, similarly affected as before. It began fourteen days before, and the joint was much swollen, as was also the bursa. I directed the same means to be employed as in the former attack. In a few days every thing was as it ought to be, and the joint has remained healthy ever since.

There was another man, æt. 70, who had a great number of knotty glandular swellings on the neck, and on the lower edge of the under jaw, from the size of a pigeon's to that of a hen's egg; they were indolent, free from pain, and circumscribed, and had a doughy feel. I caused him to use the same remedy, and in five or six days the swellings had nearly disappeared. I gave him besides internally one grain of sulph. arsenici morning and evening for some weeks, by which means he was perfectly restored to health.

The employment of this remedy naturally requires some caution. It acts very powerfully, and causes a sensation of burning on the skin, and often after two or three rubbings, sooner or later, it produces an eruption on the skin like measles: afterwards the skin crumples into yellow, parchment-like looking scales, as if burned. It is necessary that the patient should avoid applying the acid to such parts as have a very thin cuticle, as the lips, the edge of the nostrils, or to the eyes. When applied for pain in the head or face, the patient should close his eyes firmly, and keep them so for some time after the rubbing in was finished. I generally make use of a drachm of the mixture poured out in the palm of the hand, for rubbing in at one time. Rarely can it be rubbed in more than six times consecutively, because the skin becomes excoriated,

and an unendurable burning pain arises, but by leaving an interval till the reaction on the surface of the skin has ceased, it may again be proceeded with. It is also necessary to caution the patient not to let the liquid fall on his garments or bed-clothes, as it will burn them through, and not to bring a light too near in the evening, as it will readily catch fire.

I have no doubt that this remedy would be found of use in diseases of joints, and in varicose veins, in a weaker degree of solution in the latter case, although I have not tried it in those cases as yet. Trial might also be made of it in cases of encysted tumours and ganglia. I have cured a dry lichen with it within these few days. Its useful effects on genuine phlegmonoid inflammatory swellings are extraordinary, but I have never found it useful in deeply situated inflammations without causing excoriations.—*From the Medicinische Zeitung.*

Incontinence of Urine cured by Strychnine.

A linen weaver, of sound constitution, who had hitherto been in good health, was in the habit of having himself bled every spring, in order to remove a sensation of weight in his limbs, with fulness in his head, and a feeling of distention in the abdomen which came on at this period. In the year 1836 he omitted the bleeding, whereupon pain in the back, with fulness in the abdomen and headach, came on. This man, who hitherto had been of a tranquil disposition, now suffered some losses in his trade, which troubled him much. Shortly after this, in the month of July, he was affected with lightness in the head as he stooped to arrange something, and fell senseless. He was carried to bed, where in half an hour he came to himself. From this period his urine flowed from him involuntarily. A surgeon bled him, and prescribed Glauber's salts and purgatives. His state of health improved by degrees, insomuch that he was able to go about and employ himself a little, but the incontinence continued, by which the man was brought to a very melancholy and depressed state of mind.

Dr. Behrend, the district physician, was called in. He found that there was paralysis of the sphincter muscles of the bladder, on which account the urine constantly dribbled out: his powers of motion were weakened, his appetite tolerable, evacuations natural, and no fever. Dr. B. ordered a draught consisting of angelica root and æther, which acted so far usefully as to lessen the weakness in the extremities: his spirits also improved: the incontinence, however, remained. The physician now administered strychnine internally: he dissolved a grain in two oz. of distilled water, and sweetened it with

sugar : of this, the patient took a dessert spoonful every morning and evening, and by degrees increased the dose to two spoonfuls : its effects were perfectly successful. After employing this medicine for fourteen days, the incontinence of urine ceased ; the sphincters recovered their power, and the patient was completely restored to health, without any sympathetic symptoms having occurred during the use of the strychnine.—*From the Beiträge zum Sanitäts-Berichte des Frankfurter Regierungs-Bezirks.*

Inflammation of the Testicle treated by Compression.

Dr. Hildebrand states, that since the publication of Fricke's paper on the treatment of orchitis by compression, he had made observations on five cases, consequent on gleet which had been too suddenly arrested by the excessive use of balsam copaiba during the inflammatory stage, by heating drinks, and by sympathetic metastasis of the inflammation. In these five cases it was the left testicle which was affected. In all, Dr. H. applied pressure by means of sticking plaster, after the manner recommended by Fricke (which we have described in a former number of this Journal), without any preparation. In two cases only, when inflammation of the testicle was very excessive, he applied six leeches, and applied warm fomentations to the part for six hours, partly to encourage the bleeding, and partly to lessen the tension of the whole scrotum. In all five cases he had seen the most extraordinary effects in the space of four or five days. The application of the compressive apparatus was not attended with the slightest inconvenience ; it succeeded equally well when the patient lay in bed on his back, with his legs well separated, or when he sat on the side of the bed, or edge of a chair. The strips of plaster employed for compression were formed from the emplastrum cerussæ, and were not placed, as Fricke directs, from above downwards, but from the perinæum upwards, each strip half covering the one next to it, and proceeding thus till they came together over the pubis : he then laid a strip of adhesive plaster obliquely across the ends to secure them. From this compression, even when very tight, he had seldom seen any great pain follow. After, from twenty-four to twenty-eight hours, during which time the patients were obliged to lie in bed, with the scrotum well supported, the strips usually became loose from the diminution of the swelling. This loosened plaster was not taken off—the doing so would give great pain—but other slips laid over it, by which the pressure may be still kept up, and increased.—*Medicinische Zeitung.*

We extract the following articles from a communication by P. J. Schneider, in Hufeland's Journal der practischen Heilkunde :—

“ 1st. *Oil of Turpentine in Neuralgia.*

“ It is well known that after the plan advised by Recamier, the oil of turpentine has been usefully employed in various cases of neuralgia. I have used it in many cases, both chronic and acute of coxalgia and ischias, in most with extraordinary good effect, and sometimes the quick recovery of those cases was quite astonishing. From my experience, the best formula for this remedy is the following :—

R Ol. Terebinth.
 Gummi Arab. ā ā ʒij.
 Sacchari albi ʒss.
 Aquæ Menthæ crisp. ʒiv.
 Syrupi Menthæ pip. ʒi.
 M. Capiat cochlearea tria magna omni die.

“ At the same time, I cause the ol. terebinth. mixed with two parts of the volatile camphor liniment to be rubbed occasionally into the part affected.

“ In recent cases, the affection yields unusually to a third repetition of this medicine; in cases of longer date and more deeply rooted, it must be repeated oftener, and continued for a longer time.

“ In very chronic rheumatism and gout, with congestion and torpidity of the abdominal organs in lymphatic and sanguine constitutions, no remedy will be found more useful than the following recommended by Stark.*

R Pulv. G. Guaici ʒij.
 Flor. Sulph. ʒi.
 Calomel ʒj.
 Pulv. radicis Ireos florent, ā ā ʒss.
 Opii puri, gr. ij.
 Sacchari albi ʒss.
 M. fiat pulvis exactissime.

A tea-spoonful of this powder to be taken morning and evening in a glass of water. In order to render its effects more cooling, or purgative, addition may be made of nitre, or of the leaves of senna.

* Dr. J. Chr. Stark's Handbuch zur Kenntniss und Heilung innerer Krankheiten.

“2nd. *Corrosive Sublimate in Syphilitic Pains in the Bones, in Rheumatic or Gouty Complications.*

“According to Burdach, the sublimate is a genuine specific in rheumatic gout in the following form :—

R. Mercur. sublim. corrosivi, gr. ij.
Aquæ Cinnamomi ℥iss.
Vini Seminis Colchici ℥ss.
M. Capiat gtts. xxx. ad L. omni die.

“In numerous cases in my practice this mixture has produced exceedingly quick and enduring good effects, and that in the most different kinds of syphilitic affections, with gouty and rheumatic complications. Van Sweiten and Frederick Hoffman have used the sublimate with the best effects in similar cases.

“3rd. *Hydrargyrum Iodinicum in obstinate Syphilitic Swellings.*

“Syphilitic swellings will sometimes, when of long standing, or when they have acquired a great size, defy the most rational plans of treatment. In the reduction of these obstinate and troublesome swellings, I have found the hydrargyrum iodinicum always produce certain and good effects. I have prescribed it thus :—

R* Hydrargyri Iodinici ℥ss.
Arungiae ℥iss.
Essent. Bergamott gtts. xv.
M. exactissime ut ft. Ung.

“Every morning and evening the swellings were completely enveloped with this ointment till they were cured, which, in many cases, occurred incredibly quickly. In large tumours it excited much pain, and, at the same time, a well regulated course of corrosive sublimate was had recourse to. This ointment was found particularly useful in syphilitic tumours in the face, on the heads of the bones, and on the extremities.

“4th. *Nitrate of Silver in Spasmodic Affections of the Heart.*

“I had many cases of spasm of the heart, irregular action, and palpitation, which recovered in a short time by a careful attention to diet, and the following simple means.

* Neues Formular und Receipt Taschenbuch V nach E de Montmahon, Von T. S. Weber. Tubingen, 1828.

R Argenti Nitratis gr. i.

Aquæ Aromat. ℥ij.

Capiat omni die bis vel ter, cochleare unum parvum; phiala bene agitata.

“5th. *Application of Secale Cornutum to Cases of Hæmorrhage.*

“Lately, the secale cornutum has been used as the best means of arresting hæmorrhage; I had an opportunity of remarking and treating the following cases.

“On the 1st of July, 1833, I was called to a strong and robust country girl, who, hitherto, had been in good health. Menstruation had occurred six days before, very abundantly, as was usually the custom with her: at this time she caught cold, immediately became sick, and, the last night, was affected with such violent bleeding at the nose, and vomiting of blood, that her death was momentarily to be expected. On my arrival, at seven o'clock in the morning, she was still bleeding considerably from the right nostril; the blood was very thin, and the patient very sallow. Pulse hard, and uncommonly quick.

“A medicine composed of aqua cinnamomi, tinct. cinnam., tinct. thebaic. and spir. acid. vitriol was found of as little use as continued cold applications to the head, neck, and breast, and sinapisms to the feet. In this distressing predicament, I ordered

R Pulv. Secal Cornuti, gr. viij.

Sacchari Albi, gr. x. M. ft. pulvis.

A similar powder to this to be taken every quarter of an hour in water.

“This patient had only taken a few powders when vomiting again came on repeatedly, and much blood was evacuated in a coagulated state. The hæmorrhage ceased after the fifth powder had been taken; however they were still continued. Then, from her great weakness, she became affected with typhus mitior, and this changed to an intermittent form, from which she perfectly recovered.

“CASE II. On the 14th of July, a countryman came to tell me, that his wife, aged 33, had been affected for five weeks with a flow of blood from the womb, which had commenced at the period of menstruation, and from that time, despite of medical aid, it not only had not ceased, but had even become more violent; she was not pregnant.

“Not having the advantage of seeing this patient, I prescribed the powder mentioned in the preceding case, and ordered that she should be kept in the horizontal position, a point I always

insist upon, with perfect quietude and the avoidance of all inflammatory nourishment. The bleeding ceased after the fifth powder, and did not return again, but the patient felt herself inexpressibly weakened and nervous. When the hæmorrhage ceased, the powders were omitted. The next day I saw her, and found her tranquil, cheerful, and not losing blood; she caused a fresh flowing of blood, by incautiously getting up towards mid-day, believing herself to be perfectly recovered: this was again arrested by the same means, but again returned when the powders were omitted, in consequence of the inconsiderate conduct of the patient. On the 16th she took some more of the powders, which brought on frequent vomiting and violent feeling of disgust, so that it became necessary to stop it, and the following medicine was ordered.

℞ Secale Cornuti ʒi. coque cum aquâ fontanæ, q, s. colat. unc. septem. adm.
 Opii puri gr. ii.
 Syrupi Cinnamomi ʒi. M.

Every two or three hours a dessert spoonful to be taken. The abdomen to be stuped with cold brandy at the same time. By the use of this medicine for some time the patient was perfectly restored.

“CASE III. A very weak and sensitive woman, whose catamenia came on with the most violent flooding, which continued uninterruptedly twelve or fourteen days, who had been in this state for a year, and who had tried a number of medicines, received from me on the 18th of July, 1833, the powder mentioned above; when the fifth powder had been taken, there came on violent vomiting, with delirium, but no cessation of the hæmorrhage. The powder was omitted, and the decoction before mentioned given, without doing any good. Again recourse was had to the powder, and six grains were given every quarter of an hour; this powder was procured at an other shop, and when ten had been taken, severe pain in the bowels, vomiting, vertigo, lightness of head, and still more violent hæmorrhage were excited, which could only be appeased by the administration of sulphate of iron with opium, but it was always induced by the ergot.

“CASE IV. A woman, æt. 22, of small and weak stature, one year married; previous to which she had been menstruated regularly and healthily. Four weeks ago, she had had a premature labour in the third week of pregnancy, from which period she had suffered from constant sheddings of blood, which induced such a state of weakness, that she could hardly walk. Still she attended to her household concerns continually with the greatest

industry ; she had good appetite, calm sleep, and regular evacuations.

“ On the fifteenth of August, 1833, I gave her the powders of ergot, six grains to the dose, of which she was to take one every quarter of an hour, with an appropriate diet. On the 17th she came to me to assure me that the bleeding had ceased on taking the second powder, and although she had walked three miles, she had found no inconvenience. I gave her twenty-four powders more, which I directed her to take in the same doses every half hour. By this she was perfectly restored, and has remained up to the present time protected from those sheddings of blood.

“ CASE V. A woman, æt, 30, in good health, hitherto robust, the mother of many healthy, strong children, suffered in the beginning of October, 1833, although not pregnant, a great loss of blood from the uterus. When the nurse had placed the patient in a horizontal position, made use of cold stupings of vinegar and brandy and water, repeatedly in vain, I was called in the evening, and found, on my arrival, a quick, small pulse, the skin ice cold and sallow, and the patient so low, that I could not avoid giving an unfavourable prognosis.

“ I prescribed at once sixteen powders of ergot, and caused seven grains to be given every quarter of an hour. After the sixth powder had been administered, the flow of blood ceased. As a precautionary measure the same powders were repeated, and taken at greater intervals, from which time the patient has been in good health.

“ Since 1833 I have tried this excellent remedy in many other kinds of hæmorrhage, and I can aver, that two-thirds of them were quickly cured by it, in one-third it proved of no use, and recourse was had to other remedies. The ergot, when given to adults in doses under five grains, for the most part proved useless, and caused when given in doses of more than eight grains, more or less violent narcotic effects. The powder acted more effectually than the infusion ; ergot which had been kept for a year, had not by any means the same remedial powers as the fresh.”

Pomade to cure Baldness.

The baldness which occurs so frequently after typhous and puerperal fevers, syphilis, inflammatory diseases, and great losses of blood, and which in its ravages does not spare the fair sex, may be cured by the following ointment.

℞ Succi Citri recenter expressi ʒi.
Extract. Chinæ frigidæ parat. ʒii.

Medull Ossium ℥ii.
 Tinct. Cantharid. ℥i.
 Olei de Cedro ℥i.
 Olei Bergamott. gtts. x.
 M. tere simul exactissime.

Before the application of this pomatum, the whole head should be well washed with soap and water, containing a few spoonfuls of cherry spirit (Kirschenwasser) or eau de Cologne, and then dried. Next morning as much of the ointment as will go on the point of a dinner knife, should be rubbed in, and this is to be repeated every morning. In from four to six weeks the cure is generally accomplished, and the head covered with strong, beautiful growing hair.

Delirium Tremens.

We have selected the following case from Dr. Schneider's communication, on account of its differing materially from the ordinary run of cases of this disease:—

“A person, æt. 50, belonging to the enlightened class of society, making a visit with a friend in the country, drank some excellent wine of the vintage of 1825. Both drove home joyful and happy without being in the least intoxicated: on the contrary, the person whose case we relate attended to his usual occupations that evening with punctuality and regularity. But on sitting down to supper with his family at 8 o'clock, he was suddenly affected with the most violent tonic and clonic spasms, and with almost perfect loss of his senses; he became drunk to that degree usually indicated by a desire to destroy every thing around.

“Three or four could hardly hold him in those frantic paroxysms which lasted eight to fourteen minutes, and were then succeeded by an interval of lethargy, of about equal duration, in which the pulse was slightly accelerated, the respiration particularly slow and snoring, and the countenance pale; when these terrible convulsive quiverings began suddenly in the extremities again, and so by degrees the paroxysm mounted to the wildest degree of madness. During the period of relaxation there was always some consciousness, but it was of fleeting duration.

“I was called to this curious case when one of the paroxysms had continued for a quarter of an hour, and I will freely confess that I felt the most painful confusion on seeing this patient. I laid cold applications on the head, with momentary but no lasting benefit, and administered the spiritus mindereri internally, giving a teaspoonful, with a drink of cold water after it.

In some minutes vomiting of a sour-smelling fluid came on, with some alleviation of the symptoms. After a quarter of an hour another teaspoonful was given him, and as the attacks grew slighter, after the third quarter of an hour another. The attacks then ceased entirely, and the patient fell into a tranquil and refreshing sleep, out of which he awoke some hours later perfectly well. He fell asleep again and slept tranquilly the rest of the night, and the next day he did not feel the least inconvenience.

“This was the third time that this patient after a similar cause had been similarly affected ; he had always led a regular life, used exercise, and could not be with justice included in the class of drunkards.”

Dr. S. records other cases of delirium tremens, and dwells on the frequent occurrence of mental hallucination in those suffering from it ; some imagining that they saw rats, mice, devils, &c. In one case a patient is described as proceeding solemnly with a crucifix in his hand, towards a shadow which he took for the devil, and having laid the cross on it cried exultingly, “ha, devil, you’ve got it.”

Removal of Five Splinters of Iron from the Eye.

“Three times I have been visited by smiths complaining that fine particles of iron had penetrated into their eyes. By the application of a horse-shoe magnet firmly on the affected eye for many minutes, I succeeded in removing the sharp particles of iron, without the least injury to the eye.”

Tinea Capitis.

“With the best success I have employed the ointment of Jasser in this complaint ; its composition is

℞ Sulph. Purific.
Vitriol. Albi, āā ʒij.
Axungiae Porci recent. ʒvj.
M. ft. ung.

With this ointment let a portion of the head be rubbed, after which in some days, cracks in the scruff occur, and it then peels off. Every eight days I give a laxative of mercury, and prescribe a decoction of the woods, by which means this disease is generally cured in the space of from four to five weeks.”—*Hufeland's Journal*, 3rd Part, March, 1837.

Ricord in the *Gazette des Hopitaux*, No. 46, thus describes his practice in syphilitic ulcers, a practice we have seen

eminently successful in his hands at the Hôpital du Midi in Paris.

In perfectly recent cases, the first indication is to utterly destroy the affected spot, in order to anticipate general infection. This may be done either with the nitrate of silver, or when the part can be easily isolated, by excision. He thinks that the ordinary practice of dressing the ulcer characteristic of syphilis with mercurial ointment is to be condemned, his first object being to lessen and alter the discharge; therefore he cauterizes the grey bottom of the ulcer, until he perceives healthy, red granulations. The caustic changes not the discharge alone, but also acts remarkably as an antiphlogistic; for example in præputial ulceration, either when phymosis exists or not; because for the most part, after three or four cauterizations the glans can be bared. After the caustic has been applied, the part is to be dressed with an astringent wash (*vinum aromaticum*), the chemical action of which coagulates the pus. This application in general excites no irritation, and after twelve or fourteen days, by means of the caustic and these dressings, the ulcer is healed. The wine, in a certain degree, constringes the parts adjacent to the ulcer, and prevents the successive extension of the inoculation. The plan pursued by Ricord is this: the ulcer is first perfectly but gently dried, then cauterized with the lapis infernalis, and then bound up with charpie of fine texture which has been moistened with wine, but not thoroughly soaked: the dressings to be renewed three or four times daily. Local baths, or moistening the charpie too much, do injury by relaxing the surrounding cellular tissue. The wine dressing is contra-indicated whenever inflammation of the ulcer is caused by it, or when it causes an increase of induration. It is necessary that the cauterization should go deep at first when the object is to destroy the infected part, where rising granulations may be prevented from growing, but when mere desquamation is to be counteracted, the caustic must be applied quite superficially, and so directed that the wound shall be only coloured a little white. It may be asked, if after the healing of the ulcer, any internal treatment is necessary. Many are of opinion that without mercurial treatment secondary symptoms will of necessity occur after the healing of the sore. Others, on the contrary, are of opinion, that these accidents happen from mercury. According to Ricord's experience they will occur in both cases; and he declares a patient to be cured, when on the seat of the primary ulcer there are no more signs of disease, *but without adding the assurance that there is no chance of secondary symptoms occurring.*

On the Treatment of Syphilitic Buboës by Seton. By Professor LEVICAIRE, of the Marine Hospital, Lyons.

Dr. L. states, that he has employed the seton most successfully. His plan is: as soon as he perceives that the bubo contains matter, he passes a strong, round, straight, long needle, carrying a thick thread in the direction of the fold of the groin. The points of entrance and exit are those at which the gland first begins to soften. He permits the seton to remain for only twenty-four or twenty-eight hours in quiet, and sometimes to promote irritation, and prevent the too rapid healing of the openings, moistens it with a weak caustic solution, and for the first days, lays on an emollient poultice. When this is no longer necessary, he dresses it with a handful of cotton (unwrought) in order to promote the exit of the matter, the adhesive inflammation, and the development of granulations. This is supported by a bandage round the loins, and exercises a very gentle, steady pressure. Dr. L. thinks every thing disadvantageous which promotes the absorption of the matter. The matter here escapes along the seton; the walls of the abscess come gradually together: the air cannot penetrate through the opening, which is small, and filled by the seton, and the seton causes a healthy action, by means of which granulations are favoured. No cicatrix remains behind, and only three or four days are sometimes necessary for the healing of the bubo.—*Bulletin gen. de Ther.* April, 1837.

Perforation of the Acetabulum caused by a Fall on the Trochanter.

M. Gama has given a case of the above-mentioned accident. It occurred in a man æt. 30, who had fallen from a height of eighteen feet, and who had suffered a severe contusion in the region of the right trochanter. There was neither shortening nor deformity, but there was very severe pain at every movement of the limb. Some days afterwards the patient walked in the garden on crutches, and said that he felt very little pain. On the fourth day symptoms of violent peritonitis, with inflammatory swelling of the entire limb, came on. Death followed on the tenth day. On the dissection infiltration of pus was found in the immensely distended subcuticular cellular tissue, from the hip to the calf of the leg. In the right iliac fossa a conical swelling arose, and extended nearly to the kidney; this was an abscess containing pus and torn pieces of the psoas muscle, and at the bottom of it the head of the femur was to be seen where it had burst through the acetabulum. The acetabulum was broken in three pieces; the smallest of these

was placed with the round ligament upon it, and unbroken on the head of the bone; the second was the horizontal ramus of the pubis which was separated from the symphysis pubis and ischium, and the third, the ischium, which was no longer connected with the ilium.—*Gazette Medicale*, No. 17.

Ossification of the Crystalline Lens.

Professor Grillo, of Naples, has communicated a curious case, that of a sailor who had suffered from gout for twenty-five years, and who from it was affected with an obstinate gouty inflammation of the eyes, opacity of the cornea, general atrophy of the eye, and perfect blindness, and both eyes were by degrees converted into white balls. This man died of apoplexy. The remarkable part of this case is, that it was not alone the external surface of the crystalline body which was ossified, but the whole substance was perfectly converted into solid bone.—*From the Observatore Medico di Napoli*.

We extract some numerical tables made at the instance of the academy of Paris, on the influence of climate on phthisis, by Marc d'Espine.

Between 50° and 60° degrees, the deaths by phthisis may be estimated at only 53 in the 1000. From 50° to 45° the estimate is much higher, for example, in Munich 107 in the 1000; in Paris 207 in the 1000; in Berlin 71; London 236. Vienna 114. From the 45th to the 35th degree the deaths by phthisis appear as numerous. At Marseilles they form $\frac{1}{4}$ th of all the deaths; at Philadelphia $\frac{1}{8}$ th; at Nice $\frac{1}{7}$ th; at Geneva $\frac{1}{8}$ th; at Naples $\frac{1}{8}$ th; again at Mailand and Rome, only $\frac{1}{20}$ th. Still further evidence is adduced, that in the neighbourhood of the equator, there are localities in which phthisis occurs frequently, for example, amongst the negroes of the Antilles. It occurs also very frequently at Madrid, Lisbon, and Gibraltar; on the contrary, on the opposite coast of Africa, it is almost unknown. In the Mediterranean Archipelago it is said to be very common, so that those Englishmen who are predisposed to it, generally fall victims when they remain long there. The military tables of the mortality of this disease in the north, south, and middle of France, according to M. Benoiston de Chateanneuf, which extend over a period of six years' observation, are as follows: Of those soldiers raised in the North of France, there were 3742 deaths, 296 of which were from phthisis.

Those raised in the middle of France, 7155 deaths, of which 526 were of phthisis.

Those in the south were 4375, and 351 of phthisis. From

this it would appear amongst the French of the southern provinces, there is less predisposition to consumption, than in north or central France.

On the Proximate Cause, and Radical Cure of Varicose Veins of the Leg. By RIMA.

This treatise of the surgeon in chief of the hospital at Venice, is founded on the observation, that the real proximate cause of varicose tumours in the lower extremities is a reflux motion of the blood in the veins, for instance, the blood from the femoral vein retreats into the saphena, and is driven backwards from the groin towards the foot, by a power peculiar to these veins. Signor Rima has come to this conclusion from the following facts :

1st. When the surgeon removes a portion from a varicose vein in a living man, the blood is seen to spout from the upper end, as it does from an artery, as many operations of Rima, Monteggia, and Paletta have shewn.

2nd. In those persons in whom varices have been caused by wearing too tight garters under the knee, the veins are remarked to be more distended above the band than below it.

3rd. When the operation for varix is performed either by the ligature or excision, those varices which are situated below the ligature or incision are seen to contract and finally disappear, whilst those situated above, remain stationary, or increase, which would not be the case if the blood in these vessels flowed, as it usually does, from below upwards, as the weight of the column of blood from above acts in such a manner as to paralyse the valves of the veins, and thus keep up the communication between the individual varices.

4th. And lastly, the pathological anatomy of varicose veins exhibits in the lower extremities, hypertrophy of the walls, and a structure like that of the arteries.

This view of the disease naturally leads to important practical results : viz. that when about to operate, we should always select a part above the varix ; in fact, that we should approach as near as possible to Poupart's ligament.

Signor Rima thinks that inflammation following on a passive dilatation of the weak venous parietes between every two valves, with the weight of the column of blood from above, may be the cause of this hypertrophy ; from this chronic inflammation thickening follows, and a change of structure.

Whatever may be the truth of this explanation, we refrain from judging at present. The results of thirty-four operations

by excision of an inch long of the vein above the first varicose swelling, is given as follows :

Radically cured	10
Much relieved	13
Slightly relieved	6
No relief	2
Death by phlebitis	2
Still under treatment	1

Total 34

Signor Rima, after excising a portion of the vein, makes use of simple compression to stop the bleeding, and avoids using the ligature through fear of phlebitis.—*Giornale Med. di Venezia*.

Case of General Induration of the Skin. By Dr. FANTONETTI.

Antonia Alessandri, a peasant of Mortara, 30 years of age, married, and of robust constitution, came to the Medical Hospital at Pavia on the 7th of July. Menstruation had commenced at the ordinary period. She had married young ; before that period, except slight rheumatism, she had not had any illness. Shortly after marriage had an attack of scarlet fever. Convalescence was tedious, and during it, there occurred upon the skin, here and there, erythematous spots of different sizes, some two inches in length. These remained constant. After this she miscarried twice, and had two easy accouchements ; in the latter the lochia flowed very sparingly, and her legs swelled ; the œdema soon extended to the whole body, and large and painful pustules formed on various parts of the body, particularly on the breast, legs, and back ; on scratching these pustules, matter flowed out, and a tumour remained, which under the application of a mild ointment, cicatrized. The following spring she was able to share in the labours of the field ; however, soon after the skin began to grow brown and hard. She continued her labour, notwithstanding, and still suckled her child. The affection increased until she was unable to leave her bed ; some cooling remedies were administered, which proving of no avail, she was brought to the hospital.

When she entered, her skin was of dark colour, tense and hard as leather, with exception of the face and the areola around the nipples of the breast. There was still a secretion of milk, and the areola, from its softness, projected out over the nipple. The fingers, toes, joints of the arms, wrists, and knees, could hardly be moved. It was difficult to stick a needle into the skin, but when it was accomplished, blood spouted from the

aperture. Irritability was not decreased, and the temperature of the skin was natural. This tension of the skin was most painful to the patient. The tongue was loaded, a bitter taste in the mouth, some constipation, scanty urine, respiration free, pulse natural. The sensorial organs undisturbed, and free from pain. The white spots were still to be seen. Perspiration could not be induced, even by the vapour bath. Baths were now given her, to which some decoct cicuta had been added, with purgatives, and she was ordered, as drink, a decoction of dulcamara. Under this treatment, the skin became, by degrees, somewhat softer, and the right leg alone remained hard as wood. Eight leeches were applied to it, and the baths were persevered in. On the 18th, in addition to this treatment, vapour baths were ordered, and on the 23rd the skin had nearly returned to its natural condition, with exception of the leg, which was still somewhat hard. Mercurial ointment was rubbed into it, and the cicuta baths proceeded with, and on the 30th she was recovered so far as to be able to resume her usual occupations; three months after this all parts of the skin were soft, and of their natural colour, with the exception of that of the right leg.—*Ephemeride della Scienze medeche dá Fantonetti*.

Effects of Removal of the Ovaries from Cows.

M. Isell, in the *Mémoires de l'Académie Royale de Sciences, &c., de Metz*, mentions a curious fact with respect to the removal of the ovaries from cows, on the vessels secreting the milk, that after the operation, the cow will continue to give milk for many years. Hitherto, it has been rare to attempt the operation on any but heifers which have never calved. From M. Isell's experiments, the best period for the operation is that at which the milk is in the greatest abundance. The cow should have calved thrice, and the operation should be performed from the thirtieth to the thirty-fourth day after the calving, when the animal is in full strength.

The Cyclopædia of Anatomy and Physiology. Edited by
ROBERT B. TODD, M. D. Part XIII.

AFTER some temporary delay, caused by the failure of his former publishers, Dr. Todd has been again enabled to proceed with this valuable work. The present Part contains the following articles:—Gasteropoda, by Mr. Jones; Gelatin, by Professor Brande; Organs of Generation, by Mr. Jones; Ge-

neration, by Dr. Allen Thompson; and Gland, by Mr. Grainger. As usual, the text is embellished by a great number of woodcuts, explanatory of the structure of the organs described. We cannot speak too highly of the merits of this Cyclopædia. Its discontinuance would have been a national loss. It supplies the place of a whole library on anatomical and physiological subjects, and is calculated to enlarge the sphere of medical knowledge, while, at the same time, the information which it conveys is precise and accurate. We feel peculiar pride in the fact that the learned Editor is a native of Dublin.

An Essay on the Antiquity of Hindoo Medicine, including an Introductory Lecture to the Course of Materia Medica and Therapeutics. Delivered at the King's College, London. By J. F. ROYLE, M. D., &c.

WE have received much pleasure and instruction from the perusal of Dr. Royle's learned and highly interesting Essay. We think that he has clearly succeeded in proving the antiquity of Hindoo medicine, and materia medica. The researches he has made in India, and the concurrent testimony of many ancient writers, all confirm our author's conclusions, and demonstrate that the Arabians, Egyptians, and Greeks successively derived their first rudiments of medical knowledge, and the principal articles of their materia medica, from the East Indian peninsula. Dr. Royle has succeeded in ascertaining the Sanscrit roots of a great many articles of the materia medica, and has extended his inquiries to the antiquity of the Hindoo sciences in general. We have neither time nor space to dwell upon the various subjects he so ably handles. All we can do is to recommend the Essay most anxiously, both to the medical and philological public.

Dissertatio Inauguralis Medica de Typho Exanthematico, Halis Epidemio. AUCTOR JULIUS STABEROH, Berolinensis, 1834.

WE have read Dr. Staberoh's Dissertation on the Maculated Typhous Fever, which raged at Halle in 1833, many points of which are well worthy the attention of those who instruct on the subject of fever. The symptoms and the remote causes, as described, are very similar to those which distinguished the epi-

demic in this city of the same year, and the principal differences consisted in the greater frequency, at Halle, of inflammation of the glands of Peyer, and the more frequent occurrence of the disease in infants and very young persons. There is a good deal of ingenious disquisition on the physiological rationale of the maculæ which appear, in all their degrees, from the simple marbling of the skin from cold, to the blotches of disorganized matter which occur in the last stages of the worst forms of adynamic fever.

Roseola and scarlatina, of necessity, are considered in a physiological light in this little treatise. The author, although unhesitatingly allowing the contagious nature of typhus, states his opinion, that peculiar circumstances, such as being exposed to mephitic exhalations, from animal or vegetable decomposition, are capable of producing it. From tables which he had formed of cases of contagion, he had found that those convalescents whose skins desquamated rapidly, were always those most capable of communicating contagion, whilst many labouring under maculated typhus seemed to have no power of transmitting the disease.

There are many remarks in this thesis which shew the writer to be a person of observation and nice discrimination, and which give a fair promise for the future excellence of its author.

S. L. L. B.

Grundriss der Pathologischen Semiotik. Von A. F. SCHILL, Doctor der Medicin und Chirurgie, Privat Docentun an der Universitat zu Tubingen.

A Plan of Pathological Signs. By A. F. SCHILL, Doctor of Medicine and Surgery, and Teacher at the University of Tubingen.

THIS work of Dr. Schill contains a great deal of matter in a small space. It is an universal semiology, which he has divided into nine parts. 1st. Signs derivable from the nervous system. 2nd. From the senses. 3rd. From the voluntary motions, and the organs which give rise to them. 4th. From the vascular system. 5th. From the organs of respiration. 6th. From the skin. 7th. From the urinary organs. 8th. From the digestive system. And 9th, signs of puberty, and those derivable from sex.

Each of these divisions is subdivided into many others. The remarks to be found in this work possess a triteness and clearness not always the property of a German publication.

To any one studying German for medical purposes, this book would be peculiarly useful, from the number of anatomical, pathological, and physiological terms which it necessarily contains. An English version of this work would be also a desideratum, and we do not think that some of the junior members of the profession could employ their time more profitably than in the execution of such a task.

S. L. L. B.

Outlines of the Principal Diseases of Females, chiefly for the Use of Students. By FLEETWOOD CHURCHILL, M. D., Licentiate of the King and Queen's College of Physicians in Ireland; Physician to the Western Lying-in Hospital; Lecturer on Midwifery, &c., in the Richmond School of Medicine. 1838, pp. 402.

IN explanation of the author's views in the publication of the work, whose title stands at the head of these observations, the plan on which it is arranged, and the objects aimed at, we conceive we shall do best by allowing the author to speak for himself.

“ If any apology be necessary for the publication of the following work, the author trusts that it will be found in the circumstance, that a treatise on the diseases of females, adapted equally for junior and senior students, is yet a desideratum in our medical literature : many valuable monographs we possess, and even volumes of admirable essays on this subject ; but the former are so scattered, as to be out of the reach of the greater number of students, and the latter so little elementary, as to be unsuitable except for the more advanced. To meet these objections, it has been arranged, in the present volume, that the text shall contain an ample outline of the history, pathology, symptoms, and treatment of the diseases, without any detail of controversies or conflicting opinions, which are given in full in the notes appended to each page, so that the junior student, by confining his attention to the text, may acquire elementary information, which may be subsequently extended by consulting the notes and references.

“ In the notes, likewise, will be found extracts from various authors, wherever the support of their opinions seemed desirable. I have preferred giving their views in their own words, as being less liable to be mistaken.

“ Any remarkable and authentic cases, which bear upon the subject, have been inserted, for the double purpose of elucidation and description.

“ From the sketch just given, it will be evident, that the volume has no higher pretensions than that of being a compilation, with the

addition of whatever information I may have acquired from hospital or dispensary practice. I have endeavoured to ascribe each opinion to its true author, and to appropriate none that are not strictly my own."

Having thus given the author's plan and scope, we feel no hesitation in expressing, first, our approbation of the course taken, as likely to be highly serviceable to the class of readers for whose instruction our author has composed his work; and secondly, our unqualified opinion that he has accomplished his object in a manner no less complete, than the aim was judicious; wisely preferring to be useful and correct, to affecting originality in matters where originality was not to be found, he has availed himself, with no less felicity than industry, of the labours of others, in this wide field of important medical investigation; so that the book affords, at one view, the results of his own judicious observation, and the opinions of all the most esteemed authors who have preceded him.

The first part of the work treats of *Diseases of the External Genitals*; and the second of *Diseases of the Internal Genitals*; under which we have diseases of the vagina; diseases of the uterus; organic diseases; increased secretion and accumulation of fluid; inflammations, &c., lesions of nutrition; tumours, not malignant; malignant diseases; displacements; diseases of the fallopian tubes; diseases of the ovaries.

Chapter V. p. 6, gives a full and accurate account of that most distressing and unmanageable affection, *pruritus vulvæ*; we can ourselves confirm the value of the treatment alluded to, p. 9, namely, the use of sulphuric acid, from the internal use of which, and its external application in the form of ointment, much diluted, great benefit was derived in an aggravated case of this affection, not long since under our care.

The author thinks that

"Should the nymphomania become confirmed, inasmuch as the cerebral functions are involved in the complication, our treatment must not merely be local, as already directed, but, in addition, considerable advantage will often be derived from remedies directed to the relief of the nervous centre, such as leeches applied behind the ears, a douche of cold water to the head, whilst the patient is taking a warm bath, &c."

Or speaking phrenologically, we should take care of *number one*. But seriously, are we really likely to derive any advantage in alleviation of the local affection of the genitals by application to the region of the cerebellum? We apprehend not; but all do not think so: in a case of this kind treated, not long since, by a distinguished practitioner at the other side of the water, ice was repeatedly applied to the region of the organ of *amativeness*, but without any effect.

Before entering into a detail of the individual affections of the uterus, the author lays down some general rules under the title of "Preliminary Observations" which we consider as very happily put, and strongly recommend them to the attentive perusal of our junior brethren, and the consideration of all: they extend from page 45 to page 60.

Under the head of *disorders of menstruation*, we have a very full and complete account of the several irregularities, and morbid conditions to which the function of menstruation is liable; and especially of the various forms of amenorrhœa, and the variety of remedial measures which have been from time to time recommended for its treatment; we cannot, however, agree with the author, that "before we can pronounce any case to belong to the class of simple amenorrhœa, we must ascertain that the development of the uterine system is in proportion to that of the body generally, &c. &c." p. 67: if such a preliminary proceeding were to be considered essentially necessary to enable us to form our opinion as to the nature of cases presented to us in practice, we should seldom have it in our power to prescribe at all; or we should do it on what the author would obviously consider insufficient grounds; for assuredly very few young females would consent to any such investigation, nor could we indeed with propriety propose it, since, as it appears to us, a correct opinion can be formed without having recourse to any such investigation. Neither can we admit, in the absolute way in which it is stated, p. 70, that "deterioration of the general health, chlorosis, and, for the time being, sterility," are necessary consequences of amenorrhœa. In the great majority of cases the amenorrhœa must be regarded not as the cause, but as itself the consequence of the deterioration of the general health; and, secondly, we have repeatedly seen cases of amenorrhœa without any disturbance of the general health. Again, chlorosis does not always, nor even very often, follow amenorrhœa; and it is by no means unfrequently observed in cases where the catamenia are not absent; and lastly, amenorrhœa and sterility are by no means necessarily connected, numerous instances having occurred in which conception occurred during a long persistence of that condition.*

The chapters on dysmenorrhœa and menorrhagia will well repay their attentive perusal; and we abstain from here quoting several valuable passages from them, only because their contents have been already placed before the public by our author in the *Edinburgh Medical and Surgical Journal*.

On the subject of irritable uterus the author says, "We are indebted to the late distinguished Dr. Gooch for the recogni-

* See Dr. Montgomery's *Exposition of the Signs of Pregnancy*.

tion and description of this disease.''' On this point we must beg leave to dissent, and suggest, that Dr. Gooch was anticipated by Guilbert, in a work entitled *Considerations Pratiques sur Certaines Affections de l'Uterus*, published in 1826, in which there is a most complete description of the affection under consideration. Neither can we subscribe to the opinion that inflammatory action is not an ingredient in the affection, having had the most unequivocal proofs of undue vascularity of the organ under such circumstances, and obtained the most decided benefit from the abstraction of blood directly from it and the adoption of other measures calculated to subdue a low form of inflammation.

Our author's account of uterine leucorrhœa we consider as among the best parts of his work ; the whole chapter on this subject is replete with interest, and contains many most useful suggestions both in the diagnosis and treatment of this very troublesome and often greatly debilitating affection. We should dwell on this point at more length, but that our author's views, as formerly laid before the public,* have been so generally approved of as to render any further commendation on our part unnecessary.

In the subsequent chapters of this work are discussed several highly important topics under the heads of organic diseases, inflammations, &c., lesions of nutrition, tumours not malignant, malignant diseases, displacements, diseases of the fallopian tubes and ovaries.

From entering into an analysis of these we are for the present prevented by want of room, owing to the unusual quantity of space devoted to original communications in our present number ; but we consider their execution as fully equal to the parts already noticed ; and before concluding our observations we wish to repeat our conviction, that, while our author has rendered to the class of readers for whom his work was intended, a most acceptable service, and of which they will do wisely to avail themselves, his senior brethren will turn to his pages with satisfaction, and find in them such a store of references to sources of valuable information, as will at once save them the weary toil of many hours' tedious research, and impress them with a conviction of and respect for the industry, information, and ability of the writer. For ourselves, we sincerely wish the work all the success which its intrinsic merits deserve, and should consider our collection of books imperfect were there not to be found amongst them Dr. Churchill's *Outlines of the Diseases of Females*.

* Edinburgh Medical and Surgical Journal, No. 121.

A Lecture on the Nature and Cultivation of the Medical Profession, intended as a Guide to Students. By GEORGE T. MORGAN, A. M., Lecturer on Surgery, in Aberdeen.

WE have read Mr. Morgan's lecture with the greatest pleasure, and the interest we felt in it was not a little enhanced by the strong and sound religious principle which is inculcated throughout. There shines forth from it also, a kindliness and warmth of feeling towards his fellow man, as honourable to the lecturer's goodness of heart as it is rare to be met with in these publications which are supposed to be the organs of general medical opinion at the present day.

Commencing, the Professor gave a short sketch of the history of medicine, the variety of sciences which it embraces, the knowledge requisite to make a good physician, with the insuperable bars to that knowledge in many of the fields of inquiry. The author then enters not unphilosophically into a disquisition on the hypocrisy of the human mind, in attributing those faults to our profession, which are really our own, and asks, how few of us spend our time as we ought to do? How few of us, alas! labour in our vocation? We rest contented with the doctrine of others: we leave what is unknown, or not understood, to be revealed by the enterprising few, &c.

We are glad to find, also, that the old notion of a liberal education being necessary to what formerly were called, with justice, the "learned professions," is, in this lecture, strongly insisted upon.

"On the necessity of a liberal education, previous to entering on the study of any of the learned professions, I need not on the present occasion enlarge. To you who have enjoyed, or are now enjoying this, its advantages must at once appear inestimable; and while we would readily concur in the adoption of some measures for preventing ignorant individuals being found amongst us, we would not be understood to sanction too hastily any which might tend to shut out the humble student of merit. A classical education can now be so easily obtained in this country, through endowments and other means, by those not possessed of the pecuniary resources within themselves, that I can no longer see reason why it should not be insisted on as a preliminary to the higher departments of science." "You are not to entertain a hope of understanding your profession by attendance on lectures, or a mere acquaintance with books, howsoever extensive the latter may be; it is, as I have said, to the study of nature that your efforts must be constantly directed, however difficult her language may be to interpret. This is the true spirit in which you must pursue your investigations; this is the means by which the territory of human knowledge can alone be enlarged; and although each of us, during our lives, should, to use the words of the immortal

Newton, but resemble ‘a child gathering pebbles on the sea-shore;’ I say, although our knowledge should be confined to a mere outskirts of the field of discovery, compared with the vast labyrinth which we have to explore, yet it is only by seizing on whatever contributions nature throws at our feet, and by an humble submission to all the intimations of observation and experiment, that our researches will be rendered permanent, and our learning useful.”

And here the author quotes John Hunter as an example of one who, from the lowest obscurity, raised himself by his own persevering efforts to a reputation, wide as the world itself, and enduring, whilst the age in which he lived shall dwell in memory. The reason assigned for John Hunter’s greatness by Mr. M. (and fully do we concur with him), was his strong love of truth, and avoidance of speculation.

“Other speculators had consulted the book of nature, principally for the purpose of seeking in it the defence of some favorite theory, partially, therefore, and hastily as one would consult a dictionary. Hunter perused it as a volume altogether worthy of being studied for its own sake; and hence proceeded both the patience with which he traced its characters, and the rich and plentiful discoveries with which the research rewarded him.”

Anatomy is regarded by our author as the key-stone which binds the different branches of the medical profession together; and he recommends it as the first study of medical education, then physiology, and later chemistry and botany, although he seems to consider this latter science more as one of the accomplishments of the physician, than of his necessary acquirements. Mr. M. is rather too severe upon the system builders of the age, when he asserts that what is called *practical* medicine is for the greater part “a mystery.”

“And were we to go to work and dissect that fabric with minuteness, we should discover that it was but a hideous mass of human folly,—a pile reared upon error and invention, beneath the ruins of which lie interred all that is to be considered valuable of it as a science.”

However, we will agree with him when he advises the student “not to store up his mind with the incongruous classification of Cullen, or wander blindfold through the philosophical labyrinth of Mason Good.” That our author does not give implicit confidence to that uncertainly arranged number of facts which its votaries are pleased to denominate the *science* of phrenology, may be adduced from the following query:

“What would the physiologist give to be able to assign a use for the individual portions of the brain, to be admitted into the secret of the nervous power, &c.”

But the most spirit stirring and ennobling sentiment expressed in the whole lecture, and which pleased us best, was the following,—the last quotation our space admits of our making.

“ Truth, indeed, should be the grand object in all our researches, to the attainment of which every thing else must be willingly sacrificed. Some men say that ‘wealth is power,’ and some that ‘talent is power,’ and some that ‘knowledge is power,’ and others that ‘authority is power;’ but there is an apophthegm that I would place on high above them all, when I would assert that ‘truth is power.’ Wealth cannot purchase, talent cannot refute, knowledge cannot overreach, authority cannot silence her; they all, like Felix, tremble at her presence. Cast her into the seven-fold heated furnace of the tyrant’s wrath,—fling her into the most tremendous billows of popular commotion,—she mounts aloft in the ark, upon the summit of the deluge. She is the ministering spirit, which sheds on man that bright and indestructible principle of life, which is given by its mighty author, to animate, to illuminate, and inspire the immortal soul, and which, like himself, is ‘the same yesterday, to-day, and for ever.’ When the mould has long been heaped on all the pride of wealth, and talent, and knowledge, and authority,—when earth and heaven itself have passed away,—Truth shall rise, like the angel on Manoah’s sacrifice, upon the flame of Nature’s funeral pile, and ascend to her source, her haven, and her home—the bosom of the holy and eternal God. From the most violent conflicts of opinion, truth has nothing to fear; though long to us, to her a thousand years are but as one day—a point—a nothing—in the eternity of her duration. Oppressed amongst us beneath the chaos of human follies and errors, she must, she will emerge unhurt at last, unchangeable as her author. By the mere force of durability, she must ultimately stand alone, solitary amidst the wreck of those perishable materials by which for a time she is overwhelmed:—‘*as the ark floated in the midst of the waters.*’ To her, the living spirit of philosophy, immutable, immortal, infinite, eternal Truth,—to her, parent of all knowledge, fountain of light,—to her may be addressed, without perversion or hyperbole, the sublime apostrophe of the poet:

‘ The stars shall fade away, the sun himself
Grow dim with age, and nature sink in years,
But thou shalt flourish in immortal youth
Unhurt.’ ”

The rules laid down for professional conduct, and the advice given to the young practitioner, with regard to the kind treatment of the poor, are equally worthy both of the head and heart of the author; of whom we must now take our leave, hoping that it will next year be our pleasing duty to record a similar and equally well merited eulogium.

S. L. L. B.

THE
DUBLIN JOURNAL
OF
MEDICAL SCIENCE,

1 MAY, 1838.

PART I.
ORIGINAL COMMUNICATIONS.

ART. VIII.—*A Letter on the Metaphysical and Phrenological
Opinions of Doctor Elliotson.*

TO THE EDITORS OF THE DUBLIN JOURNAL OF MEDICAL SCIENCE.

GENTLEMEN,

A stranger to your profession, though one by whom it is very sincerely respected, I am not quite sure that I am not presumptuous in claiming a space in your valuable Journal for some observations on a work of one of your professional writers, Dr. Elliotson, whose reputation and talents tend to make his errors, if any he commit, a fit object of critical animadversion. I venture, however, to solicit this favour, because I believe your columns are open to an inquirer after truth, whoever may be the inquirer, provided the subject-matter of the inquiry be worthy of public and professional attention.

I have been much amused by, and in some instances have acquired very interesting information from the Doctor's last work on the *Physiology of Man*. His industry and learning, in the

proper province of his professional studies, are as indisputable as they are reputable to him ; and I undoubtedly should have confined myself to the character of a grateful reader rather than that of a reluctant critic, if I did not feel that he has, in one or two instances at least, deviated from the peculiar line to which both professional studies and regard to public interest would seem to invite him.

In fact, when in the course of my desultory reading, I first took up his *Physiology of Man*, I began to entertain an apprehension that he would soon be tempted to wander beyond the limit within which only, it would seem, the certainty of truth can be found by the curious speculator on man's intellect ; that he would desert the dry and narrow, but more profitable labour of the anatomist and demonstrator of the physical frame of man as it is found among men, whether *living* or *defunct*, and be allured to expatiate in the boundless, fanciful, and profitless region of, what perhaps I may not improperly call, medical metaphysics. I am sorry my anticipations have been verified : for we find him now, not pursuing attainable and useful truth through successive and well-proved experiments upon the human subject, its bones, muscles, nerves, the composition and functions of its brain, spinal marrow, &c., but in pursuit of that *ignis fatuus* of metaphysical speculators, the *materiality* or *immateriality* of what is called by the mass of mankind, and generally by the Christian world, the human SOUL. And next, as if it were not enough for his mighty mind to grapple with this intangible, ever-flitting, elusive, evanescent, never-to-be-fixed, and *most unsubstantial* subject, he has also taken the field against the common sense and affronted intellect of mankind, in support of a little knot of noisy *philosophists*, well-meaning, easily-satisfied reasoners, who take for *proved* every thing that is *plausibly stated*, and regulate their philosophical creed on the principal of the old saint, whose name I now forget, viz., "*Credo quia impossibile est.*" You perceive in a moment that I mean the *phrenologists* ! It is on those topics in the doctor's

work that I beg permission to offer a few remarks ; not to *disprove* the boldly avowed doctrine of Dr. Elliotson, *that an immaterial soul is nothing more than a Centaur, or a Briareus, but to argue* that it would be much safer to admit that this question is beyond our intellectual reach, and can never be settled. Neither do I undertake, against the phrenologists, to *prove a negative*, and that the structure of the skull is *not* a sure index of the moral qualities of man, but to suggest my reasons for refusing assent to the extraordinary, and to me, indeed, incredible propositions on which those gentlemen are so obstreperous and confident.

Let me begin, then, as to this assertion of Dr. Elliotson, that the soul is purely *fabulous*—as much so as the *Centaur* of the ancients—by observing that the Doctor enters into this dispute most gratuitously. It has no imaginable bearing upon any one of the questions or uses to which his professional labours tend, or about which they are properly conversant. What, then, can have been his motive in devoting so much of a work, otherwise valuable, to an endeavour to unsettle, if not to insult, the public opinion, may I not say the creed of his country ? I am, as much as any man can be, for freedom of discussion and for truth, whatever may be its result, because I am persuaded that ultimately all truth is useful ; but I cannot patiently bear that men of talent, who must, as such, be men of influence also, should, without any apparent good, attempt to disturb the settled opinion of society on points which, so far as they at all influence mankind, do so for *good*, not *evil* ; and this, too, when it not only is *not certain* that the popular opinion is false, but *it is certain*, (or in the highest degree probable), that it *cannot be established to be so by those who arrange themselves against it*. Surely this gratuitous labour is worse than futile ; it is mischievous ! But lest I should mistake Dr. Elliotson's meaning on this subject, I give the following extracts, in his own words :

“ I consider that a *soul* stands upon the same foundation as a *Centaur* or a *Briareus*.” p. 360.

In reply to the argument for an *immaterial* soul in man, from our consciousness of personality, he says : “ The fly must be as conscious of its individual being, its *personality*, as the philosopher about whose head it buzzes. If *he* must be believed to have an immaterial and immortal soul on *this* account, so must the fly, and so must be the smallest microscopic creature.” p. 320.

He then cites an opinion of Mr. A. Carmichael, not very intelligible to me, but very conclusive, it seems, to Dr. Elliotson : “ that as *all* nature is *one* whole, all *other* created beings are also organized. They and we are in but one spot at a time, and can move from one spot to another. What does so, cannot be else than matter, and a property of matter.” p. 365. Dr. E. adds, “ I consider this *alone* a proof, that *we* possess no such *imaginary thing* as an *immaterial* soul !

Now good reader ! does the history of *bad* logic, or the *reasoning* of the *grave digger* in *Hamlet*—nay, can fatuity itself shew any thing comparable for absurdity to an inference like this, drawn from such premises ! One thing only can be even guessed at from the doctor’s declamation, and that is, that it is not expressly levelled against the human soul as a *conscious* being, to *live hereafter*, but against an *immaterial* soul.—The *insinuated* distinction, however, cannot avail him—it is too summary a mode, and not a very ingenious one, of evading the main question ; that question being substantially *this*, and this *only*, namely, not whether there be or be not an *immaterial* soul, that is, a soul divested of all the inseparable qualities of *matter*, but whether man is so constituted by his Creator, that he shall exist as a moral and conscious entity after the dissolution of his body ; and, in a future state, carry with him his *moral and conscious identity*. This is the proposition which alone requires to be settled, either with a view to present public and social good, or to the future individual

happiness of man. The question, therefore, can never turn upon whether or not the soul of man is *material* or *immaterial*, but upon the simple question, whether in *any form* of being he shall live as a *responsible* agent, or as a *conscious* one, after the *material* part of his being ends in dissolution. The *immateriality* of the human *soul*, by which is universally meant, *that* part of the man, *which*, if any, is to survive his material dissolution, may add some, perhaps much strength to the argument which asserts his immortality: but it is by no means conclusive of the main question, however the *fact* of immateriality may be decided. This argument, then, of the learned doctor only proves him a bad logician, and that all his reasoning is founded on an *ignorantia elenchi*! an ignorance of the *true* point in debate; and therefore though the doctor should make out a strong case of *probability* against the existence of an *immaterial* soul in man, he would still be unwarranted to allege as of positive certainty, that the existence of a human soul was a *fable*. Even if modesty and a due deference for the opinion of the Christian world, and of the country which he inhabits, should not restrain him from so indecorous a course, the utmost he could do, even in such case, would be to raise a *question* on the degree of probability, whether *such* a *soul*, an *immaterial one*, does or does not exist. But in fact he could not make out even this case of *improbability* against the *future* existence of a soul responsible and conscious; because, admitting as he must, the power of omnipotence to create an immaterial being, which shall last as long as his will shall prescribe, there then ceases to be any degree of improbability whatever in the affirmation of the question, unless he assumes to share the counsels of the Deity, and to have *ascertained* that *his will* is that there should *not* be a human soul at once *immortal* and *immaterial*!

I reduce the whole of the reasoning of Dr. E. on this subject of the negation of the soul's existence to this single point; first, because I am of opinion that to this it must at last come,

with whatever talent, or to whatever length the discussion may be conducted. To be made *certain* by *reasoning* on the subject is impossible. We want materials for argument to arrive at certainty, from the want of *faculties* fit for the research, and of *facts*, from any possible experience ; and therefore it is, that revelation, or the omnipotence of the Deity, understand it as we best may, is our last resource.

But may we not go yet a little further, even on the topic of the *immaterial* soul, and extract, perhaps from himself, some concessions in its favour, hostile and ardent as he is against the doctrine. Let me begin by asking, does he deny that *any* thing immaterial exists.

1st. Is the *great first cause* material ? He will evade by saying it is a case above our comprehension, unique, &c. &c. &c.

But this, however, admits that *something* distinct from acknowledged matter *may* exist, though we may not comprehend it. Now this is all that is required, when the question as to an *immaterial* soul occurs. It may be admitted, and it is the only result of all our reasonings on the subject, that we are, from the imperfection of our faculties, incompetent to decide absolutely, that the thing which is called *mind* or *soul* is *material* or *immaterial* ; that the difficulties which beset each side of this question are such, and so equiponderant, that the human intellect, in its present state, is unable to derive from them a certain inference amounting to absolute knowledge. That *thought* is generated or produced by the instrumentality of the brain, and that we, at present, have no *certain* knowledge *how* it could be produced otherwise, may be admitted ; but rash and presumptuous indeed, must that man be, who would *deny* the *possibility* of its being produced by *innumerable* other modes, within the power of a being whom we denominate and believe to be *omnipotent*.

It is this view of the question which places it without and beyond our power to decide peremptorily, that the soul may not be *immaterial*, however strong may be the arguments which go

to shew that *thought*, which is, in one sense of the word, said to be the *essence* of the soul, is produced through a cerebral process.

But whatever difficulty there may be in coming to a decision founded on *certain knowledge* on the subject of the *immateriality* of the mind, it is certain that *thought* itself, the essence or the result and proof of the existence of mind, is *immaterial*; and perhaps this view of the subject may tend in some degree to lessen the confidence of those who dogmatically deny the immateriality of mind, or soul, itself. Let us examine what *thought* is, on the concession of all parties. I conceive it to be, in the first instance, the *production* or *result* of the *operation* of *external objects* through the medium of the *senses* or *faculties* (call them nerves, or by any other name which suits your particular theory,) upon the *brain*, or some part of the brain of man, in a sound intellectual state. The *modus operandi*, the manner in which *thought* is *generated*, or, indeed, what is properly called *animal life* itself, by the occult operation of *matter* upon *matter*, i. e. through the *nerves* upon the *brain*, is, and must for ever be beyond the reach of human observation, or intellect. Admitting, as I most readily do, that *thought* is so *generated*, and that it has pleased the Creator of man, *that by the operation of external material objects*, communicated by the nerves to the brain, *the brain should be made the immediate organ by which thought should be produced*, yet the mystery, the undiscoverable secret, *how* the brain, an heap of pulpy matter, should mediate or immediately give being to those results by which are produced the *immaterial essences* or *products* (language fails us when we come to inquire into nature's impenetrable secrets!) which we call *thoughts*—*thoughts* that become commensurate with the visible creation! which span the heavens! *measure* the inconceivable rapidity of its light! glance *backward* through, probably, innumerable ages, during which that creation has existed! and calculate *forward*, through interminable time, the motions and positions of those countless bodies

which are but a part, perhaps, of those that FILL (if it be not a solecism produced by the scantiness of human language!) *universal* and *interminable* space! Yes! the mystery by which the material organ, *brain*, produces, by the Creator's *fiat*, those thoughts that are commensurate with all time and all space, and which, moreover, produce those *essences* or beings of another kind, which we know by the names of *sentiment*, feelings, affections, passions, the exquisite and indescribable *delicacy* with which thought blends with thought, and generates *other thoughts* finally impelling as motives, to human action,—varied in all the inconceivably different results which we daily observe (and without wonder, from their constant presence) influencing the masses of our species that move upon the surface of our globe, and governing the fates and fortunes of its inhabitants!—the fact, the acknowledged though mysterious fact, that the brain is made the producing organ by which *scientific thought* and *human feelings, sentiments, affections, and passions* are brought into existence, for the purposes intended by the Creator, and by his power, surely may teach us to believe it to be within his power also to produce an *immaterial soul*! a being not more beyond *our* power to comprehend it, than is the sentiment of *pity*, of *justice*, of *power*, or any one of those abstract mental affections, which, though purely *immaterial*, we daily witness without wonder or question! Through that certain and, to us, yet unknown process, which is carried on in or by the brain, (and which is called *reasoning* upon successive impressions so received,) those several results of the operation of external objects come finally to resolve themselves into what are logically called PROPOSITIONS, affirming or denying certain modifications and relations of truths, respecting moral or physical *existences* and directly *conducive* to certain moral or physical ends or purposes. Those propositions, or truths, or rules, thus generated by the operation of external objects on the brain, are *positive, moral, and eternal* essences, perfectly distinct and separate from all *material* existences. Thus, for example, from the view of the external, visible, and material universe, com-

bined with *laws* which govern it, all collected from the impressions made by external things, through the medium of the senses, and by the reasonings carried on and completed in the brain and ending in fixed and well-proved propositions, we arrive at the conclusion, that the universe is the work of some first cause, possessed of, *at least*, that degree of power which the production of *such* an universe would require ; and, from the proofs of *unity* of purpose appearing from a contemplation of the whole, we also become satisfied that the work has been that of ONE creative intellect, &c. &c. It is unnecessary to illustrate this idea more at large. It is quite obvious that here a TRUTH, a certain assertion or proposition of the mind (i. e. the brain operated upon, &c., as above,) is ascertained, produced, generated, which *truth*, as such intelligible proposition, is perfectly distinct and abstract from *matter*. It is, moreover, once produced, *everlasting*, and *immortal*. It is an *entity* which can never cease to be, though from want of living minds to transmit, or letters to convey it to posterity, it may be unknown now, or become so hereafter to others, in consequence of not being communicated ; but it is, as an existing entity, communicable from man to man, from mind to mind, from one generation to another. It may *need* matter as a *medium* of communication, but it is perfectly and absolutely *distinct* from the *material* vehicle by which it is transmitted ; it has a *metaphysical*, though not a *corporeal* existence, and wants only a *consciousness* of individual existence to make it, to all intents, what we mean by an immaterial spirit. And who will say that it is beyond the power of Omnipotence to give to this immaterial entity, or any combination of such, this consciousness of existence which would perfect its personality and identity.

But, perhaps, I go too far, and refine too much in thus endeavouring to shew that the existence even of an *immaterial*, immortal “soul” is not an absurd or incredible proposition, or subject of belief. I do not here mean to assert that “soul” is or must be *immaterial* ; my object solely is to show that the

dogmatist who asserts that a “soul” *distinct* from the material brain and *surviving it, cannot be*, and must *therefore* be *fabulous*, fails in his proof; and that, in his endeavour to disprove the existence of a “human soul” fitted for a state of future existence, he is a *bad* reasoner; and, from the attempt to divest religion, and the virtues of social life, of the aid which the firm belief in the existence of such an entity, and such a future state, affords, is, for so much, a *bad* citizen. Nay, even give up unreservedly the *possibility* of an *immaterial* soul and its existence in a future state of being, and yet the broad proposition laid down by Dr. Elliotson, that *the human soul is a fable*, may remain equally false and dangerous; for, what is there to prevent *that* Omnipotence (to which in this and in myriads of cases we are obliged to recur in aid of our imperfect reason,) giving immortality to a modification of *matter*, and thus prolonging to indefinite duration, a “soul” possessing all the qualities, powers, and consciousness which that which the Doctor now calls *mind*, possesses and exhibits?

Will he deny that it is possible for creative power to transmit from one system of *matter* to another system, the same power and identical mental faculties, the same memory, the same consciousness,* the same general intellectual qualities, which we are obliged to admit is actually done by the Creator in every instance in which the *consciousness* and *mental powers* of the *young* man are faithfully and accurately transmitted to the *old*, after the whole of the *material* frame of the individual has been several times successively changed for *new matter*, arranged under a similar, or if he pleases, the same *identical* organization!—But I must not trespass further on your columns on this

* There is no more difficulty in conceiving that *consciousness* of former thoughts, recollections, events, &c., belonging properly to one who has ceased to be, in one form of being, should be superadded by the Creator to a *new* organized system of matter, or a recently created immaterial being, than we perceive constantly to occur in *dreaming*, when consciousness (a false one) or memory of things that never did exist, is added to the mind of the dreamer.

plain case, and shall only add, that it is as silly in him who admits *omnipotence* to the Deity, to deny the possibility of a "soul," whether *material* or *immaterial*, enduring for ever, as it is mischievous to society to attempt subverting the solid foundation on which public and private happiness most securely rests—namely, the existence of a future state of reward and punishment.

PHRENOLOGY.

This is another subject on which Dr. E. appears as enthusiastic as he is rash, and much of the blameable and irrelevant sophistry touching the soul, with which he has marred an otherwise good book, is attributable, I think, to his insane attachment to this whimsical system; a system grounded on insufficient evidence; and for which there is no foundation in nature; none, at least, by which men may judge of its truth for themselves, and on their own examination.

The laws of nature, in every department of physical science, are proveable by indisputable experiment, which each may make for himself, if he has the ordinary means, and sufficient learning. But what are the proofs of phrenology? We admit that the brain is divisible into compartments; that these generally, and some of them more immediately and particularly, are connected with and necessary for certain functions of animal life; and that according to the perfection or imperfection of these organs, if you please to call them so, their functions will be well or ill performed. Nay, we may go so far as to say, that, assuming we have found that organ or those organs which are necessary or appropriate to *intellectual* operations, the mind of the individual having such well constituted organ of intellect will excel in mental operations, will have soundness of understanding, clearness of idea, comprehension, &c. But between this correspondence of *intellect* with its *organ*, and the *assumed* correspondence between the *cerebral* subdivisions and the *mental* and *moral dispositions* which the phrenologist so perti-

naciously obtrudes upon the public, there is not the slightest proveable or imaginable correspondence or connexion.

See again the obvious and striking difference between the species of proofs on which the two classes of cases depend ; that which relates to shewing the *aptitude* of the organ for the *physical function* it is calculated to perform, and the aptitude of the phrenological organ, for the *moral results* with which they tell us they are connected.

In the first case, the aptitude appears on the view connected with experiment. In the other, there is no possible apparent or conceivable fitness between the organ and the *moral* effect it is said to produce. What fitness, for instance, is conceivable between the organ of music (or take any other phrenological organ,) and the *moral love* or *aptitude* to excel in music ! This observation may be extended throughout the whole range of what they call “ phrenological organs.”

But the Doctor may tell us, that though he cannot show any *fitness* in these instances *a priori*, yet an uninterrupted correspondence between the organs and the moral results may be proved by experience *a posteriori* ; and that an extraordinary talent for music, or love of it, is *always* found to be *coexistent* with the organ. But how can this species of proof be had ? It must be *as extensive as our species* ; and of the extent of it, however short of the species it may be, we can have nothing but the loose conjectural proof collected from a small number of individuals who may apply themselves to the task, and whose testimony may be in every particular instance impeachable, either from want of capacity to judge, or from want of sufficient extent of experience, or from being prejudiced either for or against the system, &c. &c.

I venture to assert, that there is not to be found a *single* case in the *whole history of phrenology*, in which the witness who is produced to prove a general fact *necessary* for the system, would not be found, from some one cause or another, exceptionable either from defect of *known* character for *truth*,

intelligence, skill, opportunity of knowing, capacity in judging, want of sufficient experience, or for prejudice as a disciple, &c. &c.

In addition to all this ; even if the witnesses were *unexceptionable* as to proving apparent correspondence between the *organ* and the *faculty* which it is said to produce, it would yet be indispensable to prove that the *faculty could not* have been produced *by some other cause* than the existence of the organ ; by some *moral, accidental, extrinsic* cause.

There would remain yet another head of *objection*—and that is—that the *results* alleged as flowing from the organ are *moral* results ; and I should be glad to know whether Dr. E., or any other phrenologist, will undertake to demonstrate that the *physical structure* of an organ in the brain must or can be the cause of the moral or mental disposition which, according to their system, it indicates ? Or in other words, whether the mental disposition must not be the result of moral and not organic causes ?

Take *avarice*, or, as in their gibberish it is called, *acquisitiveness*, for instance. This vice consists in an excessive or marked *attachment* to the accumulation of wealth. Will they prove not only that the organ shows that the individual will *accumulate*, &c., but that he will do so *without a reasonable moral motive* ? That either his past history, his situation in life, or his reasonable hopes, or his rational fears, may not be the motive for, i. e. the *cause* of accumulating ? Or will they assert that the organ designates the absolute *fact*, that he *will* accumulate ; and that, independent of moral causes, the indication of the organ will be verified ? If this be what the organ designates, it is an organ showing prophetically a *fact*, not a *disposition*. If so, we may have organs designating, like fortune-tellers, (indeed the whole class of phrenologists seem to rank in this respectable class,) that a man shall die unmarried, shall accumulate wealth, commit homicides, &c. &c.

Not to pursue this farther, I content myself with saying, that unless the phrenologist can, from the *cranium*, ascertain the

quantity of *moral* effect that *may*, nay *must* be produced by the organ upon the mind of the individual, independent either of education, religion, example, or the various situations in which Providence may place him, the pretended science should rank among the *lowest* of the *low arts*, by which we so frequently see men elevate themselves into temporary notoriety ; for, in such case, of what use is the knowledge ? or by what proofs can it be tested ? And I for one would adopt the opinion which Jeffries is said to have published, though I am no eulogist of the man, “that to enter on a particular refutation, would be to insult the understanding of readers, as Gall’s opinions on the *functions in general* of man, and on his intellectual faculties in particular, are a collection of mere absurdities, without truth, connexion, or consistency ; an incoherent rhapsody, which nothing could have induced any man to have presented to the public under a pretence of instructing them, but absolute insanity, gross ignorance, or matchless assurance.” “Such is the trash,” he continued, “the despicable trumpery, which men, calling themselves scientific inquirers, have the impudence gravely to present to physiologists of the nineteenth century as specimens of reasoning and induction.”

How much are those observations fortified by what Elliotson himself states, as to the two apostles of this doctrine, Gall and Spurzheim. The pseudo-science began with the former, to whom Dr. E. attaches himself ; and Spurzheim was taught by Gall : but so little of positive or abstract truth was there in the pretended science, that the disciple quickly abandoned the system of the master, for Dr. E. states : “*Spurzheim gave to the majority of the faculties new names, which he afterwards changed from time to time for others long and uncouth, and still destitute of the uniformity he aimed at, some new-coined words, and some expressive of a doubtful, if not decidedly erroneous, view of the faculties !*”—p. 350. Thus Gall and Spurzheim differed : Elliotson adopts Gall ; and very properly says, “It would have been better to have waited content with a

few names for each faculty, so as to shew what was meant, and waited till the science shall be so far advanced that an appropriate name could not be difficult." And thus it appears, that at this moment the science is not so far advanced as to enable the professors of it to find *names* to shew, with reasonable distinctness, even *their own meaning* !

Again, see what is the testimony of the disciple Elliotson against his master : " Gall *began* empirically," (and I contend he has *ended* so ;) " he renounced all reasoning, and gave himself up merely to observation !" Thus the science is founded not on *truth*, or *reasoning*, but on the " empirical quackery" of *one* man " who never reasoned !" and whose reputation as a discoverer in the *art*, is disparaged by his immediate successor !

I have said that the system is destitute of proof ; this was treating it too tenderly—it abounds in proof, and that, too, arising from the absurdity of the system itself, *that it cannot possibly be true*. Instance the opposing organs or faculties, (which constitute a prominent part of the system,) which are distinct and independent, but may coexist, and counter-operate at the same time ! As instances, take from Mr. Combe's account in an early work of his, what he says of the organs of *hope* and *cautiousness*, two antagonist faculties, with directly opposite tendencies, one leading us to expect *good*, but with an absence of *certainty* ; *cautiousness*, leading us to the fear of *evil*. " Hence," says Mr. Combe, " he who has *hope* more powerful than *cautiousness*, lives in the enjoyment of brilliant anticipations ; while he who has *cautiousness* more powerful than *hope*, lives under the painful apprehensions of evils which rarely exist : " and on the same subject he says, " when this organ (*hope*) is very deficient, and that of *cautiousness* large, a gloomy despondency is apt to invade the mind" ! Thus the two organs countervailing each other, according to this hopeful science, are destined to a perpetual conflict, and according to the changeful course of victory between them must be the hope or despair, the happiness or misery of the subject victim ! But suppose

the two organs of *equal size and power*, (and as Combe himself admits the possibility of their *varying*, they may be sometimes *equal*,) what is then the result of the *two organs*? They must eventually destroy each other's efficacy, and leave neither *hope* of good, nor *fear* of evil! They will thus afford a happy proof of the *possibility* of the truth of the celebrated Irish story of the *two Kilkenny cats*, which in a mortal combat are said to have eaten each other up! Indeed the Kilkenny story has comparatively a verisimilitude which the phrenological wants—for it is said, that in the case of the *cats*, the “*tails*,” though the *tails only*, were left! The absurdity of the system in this respect is not confined to the cases of *hope* and *cautiousness*. There is an organ of *destructiveness* and one of *constructiveness*, chargeable with the same inconsistency—unless, indeed, without any shadow of proof or of reason, they *limit* the powers of each to certain *specific* subjects, in order to obviate the absurd result, and tell you (which they certainly may do, without assuming any *additional* confidence) that the *destructiveness* is applicable to *murder* or *homicide*, and the *constructiveness* to the making of *mouse traps* or *nut crackers*!

Pray indulge me yet for a moment to add *one* instance more to the intrinsic proofs of the utter inconsistency of this ridiculous quackery. There is, or there was, (for it is some time since I ceased to pay any attention to this *science*!) an organ called the organ of *individuality*; (it may have taken a new name lately, as impostors are apt to do;) Mr. Combe formerly treated of this organ (it had been originally *one*, but it was *afterwards* split into a *pair*) and placed it in the middle of the brow, just above the root of the nose. All the phrenologists agreed that this organ indicated *some* original *faculty*, but they were far from agreeing about the identical one. Dr. Gall at first conceived this protuberance to be the organ of the *memory of things*; that was soon found to be a little too loose, or not enough specific, and he afterwards denominated it, in order to make it more precise and intelligible, the “*sense of things*!”

but even this they naturally thought a little too indeterminate, and it then became the “*capacity* of being educated,” or the organ of “*perfectibility*.” So far, Dr. Gall laboured in vain to give precision to palpable “nonsense!” Then came Dr. Spurzheim, “*insistens vestigiis Galli*,” and divides the one organ into two; the one *above*, and the other *below*; and decided that the uppermost, called “*eventuality*,” indicated or gave the power of attending to *phenomena, facts, events, NATURAL HISTORY, and anecdotes ! ! !*” Mr. Combe concurred with Dr. Spurzheim in the division of the organ into two, and taught mankind that the *upper* was “*upper individuality*,” and the *lower*, was “*lower individuality* ;” that the upper had for its function, to give a *fondness* for *natural history*, and for memory of facts “*recorded in books*,” or “*narrated by men* ;” and the lower was “to predispose towards *observing what occurs around us*, and to take an *INTEREST in events* !” After Combe came another disciple (*Doctor*, I should call him) of the science, a Rev. Mr. Welsh, high, at the time, in character among the *craniac philosophers*, and he taught that one of the *individualities* above mentioned was merely the “organ of *motion* !” But I pass by the *Reverend* gentleman as unfit to be a compeer in absurdity with his senior professors of the science, and shall only add to the explication of those “organs of *individuality*,” given by them an account of the *uses* to which Mr. Combe, a President of the Phrenological Society, in his *System of Phrenology*, published in 1825, thinks they may be *applicable* ! After telling us in a former part of his book, that this “pair” of organs “give the *tendency to personification* or to invest abstract and inanimate objects with *personality*,” he tells us at last that—

“These organs (the upper and lower *individualities*) confer on the merchant, banker, and practical man of business, that *detail and readiness of observation* which are essential to the advantageous management of affairs. To a *shopman*

or warehouseman these are *highly useful*, and contribute to that *ready smartness* which is necessary in the *retail trade!!!*"

But he says still more ! Listen, *ye elderly gentlewomen !* if any such there be ! *He* tells you that " persons who excel at WHIST generally possess the *lower individuality large !*" Look therefore to your partners, ladies !—" the lower individuality *large !*"

And now, while we are upon this scientific and most useful theme, may we not be permitted to express our surprise, that while the propensities ("*faculties !*") of our nature are so numerous, the *organs* which niggard nature has given us are so few as those taught by the adepts in this mystery ! They are not more, I think, than between thirty and forty, enough, however, to crowd and jostle each other in our narrow *cranium*. Why have we not an organ *specifically* for *whist* ? Nay, why not for horse-racing, for dog-loving, and dog-teaching, for long-story-telling, for punning, for *lying*, a philadelphic love for brethren and sisters, as well as philoprogenitiveness ? But I must stop this enumeration ! "Proofs crowd on proofs, and still the last is the strongest." I should fill, not one of your numbers, but a yearly collection of them, if I were to go through the almost innumerable instances in which the crude, shallow, and puerile character of this mockery of science exhibits itself.

Ought I not be satisfied, and conclude here with these few, but, in my opinion, unanswerable objections to this *pseudo-science* ? I confess I think so ; but yet there is such piquancy in some of the nonsense connected with it that I cannot resist trespassing a little longer on your columns and patience. Who can resist the temptation of laughing at the amusing matter thrown in his way, courting attention—or *ridicule !*—in the *long note* of *fourteen* closely printed pages of small type, beginning with page 305 of the doctor's physiological work ; and in which

he endeavours to soften or mitigate the ridicule which he feels to belong to this *precious science* of Dr. Gall, (and, it would seem, to some discoveries of his own !) by enumerating in how many instances the world has *laughed at new discoveries*, which afterwards were found *not* to be ridiculous. The doctor is certainly entitled to use this kind of defence for Gall, his master, and to some extent for himself, as the disciple ; for undoubtedly at present there are few things more ridiculous than this *science* of the *skull* ; the professors of which ought, perhaps, with more propriety, be called *phrenetics* than *phrenologists* ! Whether contrary to all present likelihood this, at present *ridiculous*, affair, may hereafter assume a more *serious* and *respectable* appearance, *time* only, which brings strange things to light, can determine.

In the first two of those vindictory pages, we have Dr. Gall's own opinion and defence of himself ; he *modestly* introduces himself as ranking among the Grecian philosophers—Pythagoras, Anaxagoras, Democritus, nay, even the god-like Socrates is in the train ! the Stagirite, too, is among his fellows ! Among the moderns, all the philosophers of the fourteenth century, who “for their knowledge of natural things were put to death as magicians,” (the *phrenologists* are no doubt *quite safe* from *this* peril !) are introduced as his *suite*. Galileo, too, seems to come forward as chief spokesman and voucher for the importance of the science of the *cranium* ! Then, as to the *subject matter* of his discoveries, the *skull* and its organs ; he shews that *potatoes*, Peruvian bark, and vaccination, have been blessings, which, on their being first presented to mankind, were rejected with the same impious disregard of their worth, as those discoveries of the phrenological bumps ! Harvey, also, and his discovery of the circulation, Linnæus, Buffon, Bennet, Le Roy, Lavater, Descartes, Locke, and Condillac, are all paraded as illustrious examples of similar suffering and similar merit with the illustrious Gall ! After enume-

rating those celebrated names, Gall breaks out in raptures : “ This,” says he, “ is a faithful picture of what happened to *me* ! I therefore have reason to be proud of having experienced the *same* lot as men to whom the world is indebted for so great a mass of knowledge !” He, however, very prudently guards the world against being *too hasty* in looking for the *use* of this phrenological learning—“ The only answer,” he says, “ to be made to those, who, putting all things in subordination to men’s wants, ask, *what is the use of that* ? is, that “ Jesus, the son of Sirach,” had already said, “ we ought not to ask the *use* of that, for the use *will* have its reward *in due time* !”

Dr. Elliotson then takes the cue from his master, and the doctor tells us, that when *he* was delivering the Lumleyan lectures on percussion and auscultation in 1829, he reminded the college, (with a sagacious anticipation of being ridiculous, I presume,) that “ the greatest discoveries had generally been at first ridiculed, and their authors, no less than all the truest benefactors of the human race, despised and rejected by men.” It is pleasant, certainly, to one who ventures to smile at a new discovery like *phrenology*, to find, that its discoverers are thus rather pleased, and glorify themselves in being laughed at—as, the critic is thus relieved from any compunctious visitings for giving *pain* to the new benefactors of mankind !

The doctor then comes forward with *his* choral band of illustrious names, with whom he conceives himself to have some great quality in common, and names *Harvey* and *Sydenham*, the latter of whom he tells as was called a quack and a murderer, and even ventures to claim the benefit of the Horatian “ odes.” Newton also, and the great Clarke, Cassini, and Maraldi, and Handel, the countryman of Gall, ALL have in their turn, he tells us, been in disrepute with their age.—Undoubtedly they form a most respectable list of fellow-sufferers ; and peculiarly fortunate it is for the professors of the science, that whether phrenology shall hereafter “ flourish or decay,” Dr. Elliotson

and his brethren of the “skull” are sure of having the company of men of the very first order who have, like them, been unfortunate in *some part or other* of their career! But—*valeat quantum* in favour of *phrenology*—it does not follow, that because great men in former times were laughed at as *being ridiculous* when *they were not*, therefore we are not to laugh at phrenologists because *they are so*. “*Felix quem faciunt aliena pericula cautum*,” is a very sound maxim, but it never can blunt the shaft of *deserved* ridicule, that *others* have been considered *undeservedly* ridiculous before!

There is, certainly, a vein of simplicity running through some of this defence of Dr. Elliotson, as connected with phrenology, which is exceedingly amusing to one not skilled in the “*practice*” of medicine. In p. 400, speaking of his discovery of “the *almost* specific” power of carbonate of iron, he says, “for pointing out, that carbonate of iron when it failed in ordinary doses, might be given without the least unpleasant effect, in doses *ten* times larger, I was considered *little less than a fool*, and acquired a *permanent* reputation for giving ALL medicines under ALL circumstances in enormous doses, though *I* am one of the *most cautious practitioners*, and always begin in chronic cases with small doses of medicine, increasing them by slow degrees, according to necessity, and never, from my earliest days, have wished to give one grain or drop more than proves requisite. At the same time I *certainly do not regard quantity*, as *I* proceed, but *steadily augment* till the complaint begins to yield. *No practice is more irrational*, than to *discontinue* a medicine, *simply because it does nothing*, or till some effect of that *medicine* begins to appear! A gentleman from the West Indies told me he had great success with it, *but no one else gives it a trial*, and old means, which have failed again and again, are absurdly repeated!”

Might not one, who, like me, seldom resort to medicine, be well deterred from ever resorting to the faculty, by this candid avowal, that a physician may continue to try an inert me-

dicine for an indefinite time on his unfortunate frame, merely to learn how long the experiment may last before his stomach shall wince under a continually increasing quantity of an inactive drug, administered by a man who thinks no practice more *irrational* than to *discontinue* his doses, *because they do nothing !*" nay, who is so reckless of the patient, that he makes the doses *ten times larger*, until either the patient sinks, or the doses operate ! Oh Galen ! Oh Hippocrates !

The doctor next exemplifies the slow progress of *illumination* in this stupid generation, by mentioning, p. 402, "year after year have I amused myself with watching the progress of *illumination*, and comparing it with the history of the progress of the great *truths* in physical, moral, and political science. Yet not even now is it (i. e. *lighting with gas !*) at this moment universally adopted, any more than many *obvious truths*. *Darkness* is still cherished in that very spot of London, where the *greatest riches*, and the *highest rank*, both transmitted hereditarily in the longest succession, ought to have secured, with Oxford and Cambridge education, and every advantage of mental cultivation, from generation to generation, the highest knowledge and discernment. No house in Grosvenor-square has any other than the greasy, dull, oil-lamps ! &c."

What a comical proof is here given, involuntarily, of the confusion between *moral* and *physical* qualities, generated in a phrenologist's brain, by the confounding and confused dogmas of his science ! This simple gentleman is here mixing up, in this extraordinary *morceau*, the *physical light* of gas with the *mental illumination* produced by the progress of *moral* and *political* science, and (as I understand him) by "*riches and rank*" as well as "education in Oxford and Cambridge !" This is quite of a piece, and equally illustrative of *the science*, as the phrenological doctrine, that the same phrenological indication is given in the head of the *high flying birds*, as in that of the *ambitious* aspirant for *high offices* !

In the next page or two of these notes (404) we collect fur-

ther interesting matter from the simplicity of the doctor. He tells us, and it is an excellent maxim, and comes peculiarly well in a disquisition on phrenology, “*that few people have any solid reasons for the doctrines they keep such a stir about.*” Indeed this maxim, if strictly and constantly attended to, would go far to protect the simple-minded against the never-ending loquacity of the phrenologists about their *organs*, their *fundamental faculties*, their *developments*, &c. Would the time were come when those gentlemen would govern *themselves* by this advice, talk little, and make little “stir about” their doctrines, unless in proportion as their *reasons* for them were *solid*!

The doctor then gives a highly flattering portrait of his master, Gall : as a feature of it, he describes, in the language of the science, his *head*, (the face, the *physiognomy*, indicative as it is of all the higher and finer qualities of the mind, and of the tone of its passions, feelings, and affections, are beneath the notice of the true phrenologist,) “the *frontal* and *sincipital* regions of his head were magnificent!”—“his *cerebellum* was very large ; and he was *twice* married ; and though, after *separating from his first wife*, he had a *mistress*, I believe he was a model of *fidelity*, and married again immediately that his first wife died.” One is naturally led to wish, that the doctor had even so much as hinted whether he introduced the description of the “*cerebellum* very large” with the detail of the *first* marriage, the separation from his wife, his then taking a *mistress*, as a *model of fidelity* ; and lastly, his immediate marriage after his wife’s death ; it were, I say, much to be wished, that he had stated whether these *interesting* events were given with any *phrenological* view ; or whether this detail of *moral* and *physical* facts had any connexion with *organic development* ; or that this *magnificent frontal and sincipital region* were in *anywise indicative of the event or dispositions* which his conjugal life disclosed. It is, no doubt, to be deplored, that any thing connected with the organization and moral development of such a *scientific* discoverer should be lost to poste-

rity ! May we not hope that in a future edition the doctor will gratify the public in this respect ?

We now come to a nearer view of the origin and state of phrenological science. In page 405 we have Gall's own account of its *rise*. He has done *all* himself. He states, to use his own words, "with *imperturbable confidence*, that he believes himself to be above ALL his predecessors ; above all his contemporaries !" "Yes," he says, "*I* am the first who has established the physiological principles on which the structure and functions of the brain must be studied ; *I* am the first who has broken down the barrier opposed by superstition and ignorance, for thousands of years, to the progress of the physiology of the nervous system ; the first who conceived the idea of *distinguishing* the *general attributes* from the *true qualities and fundamental faculties* ; the first who determined the *instincts*, the *inclinations*, the *sentiments*, and the *talents which are connected with certain cerebral parts*. *I* am the first who had the courage, the patience, and the perseverance to examine and fix the *relations* which exist between the *energy of the moral qualities and of the intellectual faculties*, and the *various development of the parts of the brain*. *I* am the first who has extended these researches to the whole brute kingdom ; who has studied *thousands* of animals, as to their most striking instincts, inclinations, and faculties, and the configuration of their brain, both in individuals and species. *I* am the first who discovered and pointed out the *means of discovering the seat of each instinct, sentiment, and intellectual faculty*. *I* am the first who discovered these seats, and demonstrated them by numerous physiological and pathological facts, and by an infinity of researches into the comparative anatomy and physiology of all species of animals!!!" Now, gentle reader, medical, surgical, legal, historian, legislator, or philosopher as you may be, I appeal to you, did you ever read, hear, or see more of pure quackery, of silly, vain-glorious, incredible, and disgusting self-praise than this founder of phrenological science, than this *rant* of the mock

doctor discloses ; why his own merry Andrew ought to blush at it ! Why has Dr. S., whom I am far from calling either mock doctor or merry Andrew, why has he thus exposed his master's nakedness to a British public ? But let the reader go to the next paragraph, and he will find the same *dose*, the concentrated essence of vain-glorious ignorance, exhibited to his scorn. "Yes," says the Doctor Gall, "once more, *I* am the first and the only one to whom the physiology of the brain owes its existence. *I* discovered it *without the assistance of any man !*"

But however gigantic may have been the labours of Dr. Gall, his friend and disciple, Dr. Elliotson, gives us reason to fear that all may yet be lost. Dr. Gall, himself, is made to tell us, that "even he is far from believing the edifice complete ;" and then he proceeds, in the doctor's own words, to tell us what an immensity is yet to be done :—" *An immense concourse of the most favourable circumstances would be required to raise this study to the height which it is capable of attaining. There would be required a complete collection of the crania of brutes, not only of different species, but also of individuals in which qualities or faculties strongly pronounced had been observed. There would be required a complete collection of the brains of brutes, modelled in wax after nature, to multiply the means of comparison. There would be required a number of crania, or at least of casts, of men and women distinguished by some quality or faculty : finally, there would be required a more extensive knowledge of natural history than we have at present, with respect to instances of industrious aptitudes, qualities, and faculties ; in a word, with respect to the internal economy of the brute creation.*"

Here, then, we see that neither Dr. Gall's nor Dr. Elliotson's united brains appear sufficient to elaborate this science to maturity ; they want, in addition,

Complete collections of crania of brutes.

Complete collections of *all species*.

Complete collections of *individuals* in which qualities or faculties strongly pronounced *had been observed* !

Complete collections of the *brains* of brutes ; all these *modelled in wax, after nature*.

A number of *crania*, or at least *casts* of *men* and *women* distinguished by some quality or faculty.

A more extensive knowledge of natural history than we have at present, with respect to instances of *industrious* aptitudes, qualities, and faculties : in a word, with respect to the internal economy of the *brute* creation !

But now, let us suppose all this *unattainable* apparatus of brains, skulls, &c. &c., of *all* species, with *all* the stories about the history of their owners, while living, laid at the foot of the quack phrenologist : let us ask, to what immediate purpose is he to apply them ? is he about now to form and *frame* his yet disjointed system ?—his *disjecta membra* ? and *learn* from them that certain protuberances in the organs of the *brain*, or corresponding *crania*, are *necessarily*, and therefore *uniformly*, connected with certain *moral* or *physical* propensities, dispositions, &c. &c., and to ascertain whether the propensities thus said to be connected are moral or physical, or both ? If his object be, *then*, to *invent* or *discover* the proposed science, it follows that it is yet a *desideratum*, it is *not yet discovered* or *ascertained*, and Dr. Gall's glory vanishes ! If that be *not* his object, but that he intends merely to prove or *exemplify* the already ascertained science by experiment or instances, then let him produce a *specimen*, a *skull* taken from his innumerable collection : I would ask him, then, to point out the *site* of a particular organ, say *destructiveness* ; will he do so ? Is he *positive* that the owner of this skull was fierce, sanguinary, pugnacious, &c., and that he was so by virtue of or in consequence of this protuberance, and the degree of it ? He will either say *yes*, or, that the organ is not very well developed ; and *therefore* he doubts, but that the *probability* is, he was so. His *doubt* and his *reference to the probable* at once shew there is no *certain*

knowledge, and the whole is but fancy ! If he infers *positively* the mental quality from the corporeal organ, I ask for *proof*. If the skull be that of one *unknown*, there is an end of all possibility of proving his assertion by *shewing the fact*. But, let us suppose, that the owner of the skull is known, and that he was a *murderer*. Ask the quack, then, can you now shew that the murder, however well proved, was the *result* of the form, or size, or development of the organ, and *not* of a midnight broil in a brothel, whither he might have been led, *not* by his organ of destructiveness, but from his amative-ness, and its *organ*, in unison, perhaps, with his *organ* of *pride* or *self-esteem*, which produced a quarrel with a *rival* about *precedency* ; or perhaps his organ of “*love of high places*,” in a contention for the middle or the *upper* room instead of the *lower* ! or that it might not be produced by *any one of the thousand of moral causes* which produce strife, and crime, and blood-shedding among mankind ? Might not the *homicide*, too, have been accidental ? or must the *scientific* phrenologist have the previous *verdict* of a jury, or *evidence*, and the *opinion* of a *judge*, to prove that the death was malicious and aforethought, before he can bring the *instance* of the death as proof of the *character* of the organ ? But, again, may not his *parentage*, among the *guilty* and the *violent*, his *ignorance*, his *corrupting society*, his *love of plunder*, impelling him to a *robbery*, have gradually prepared him for the murder which is now attributed by this *scientific* professor solely to the *protuberance* which *he* calls the organ of *destructiveness* ?

Well ! but the phrenologist now betakes himself, for an example of his *scientific* skill, to the *cast* of some *well known character*. Is it an *ancient* cast ? Who will prove that the exact form of the living *skull* was *accurately* taken by *men*, and in *times*, to whom and in which the *science* of phrenology was unknown, and the mere skull, therefore, *not* an object of attention ? Is any proof, to rest a system upon, to be taken from the probable or possible accuracy of a *cast* made at a time when the

countenance only was considered of importance? Is it a *modern* cast? Who knows but the plastic hand of Gall or Spurzheim, or a *disciple*, fashioned it? But *suppose* the cast accurate, and to correspond with the phrenological theory; what proves the correspondence of the *faculty* with the *organ*? Is it written history, or verbal report? These can only give general details of historical facts. Is a science to rest upon the *truth* of history, as to the *precise* facts and the *motives* of them? If so, the *oxen* spoke in the times which Livy reports! Did oxen speak? If *all* history be true, the tales of Castor and Pollux, and of the *Cross* in the heavens, betokening the victory of Constantine, and that of Mahomet having had 20,000 conferences with God, while the *water* spilled from the bowl at his bed-side! Are *ALL* these true? and was Hampden at once a *rebel* and a *patriot*? &c. &c. Is the science to rest on verbal modern report? Hohenloe is said to have wrought miracles even in Ranelagh, and, no doubt, believers have been found. Are *we, therefore*, to believe those miracles? Perhaps we may do so. But if so, it is not *because* the fact of their belief has been reported or recorded. Again, if it should so happen, that in any particular case of skull, or cast examined, the *organs* and the *stories* of the disposition of the original should correspond with the phrenologist's theory, how would it appear to follow, that the *coincidence was not accidental* or *attributable* to *other* causes than the form of the organ? Might not the disposition or constitutional temperament arise from other causes operating in the brain? or in the *form*, the *strength*, or the *weakness* of some *other organ* than a *cerebral* one? Suppose, again, *two organs*, of the brain equally developed in the same head, and the cranium equally exhibiting them, how would the *scientific* phrenologist prove that it was one, and not the other, which produced the mental disposition?

Again; suppose, what must often occur, that *all* the organs in a perfectly well formed head were in full development, on what *real* or *assumed* principles could he account for a particu-

lar organ *producing* or *proving* a particular disposition in the mind or moral faculties of the individual—unless it were *philosophically* required of us to believe that the *ipse dixit* of Gall, the Master, had so decided the point? for it is abundantly clear, that even the rashness of the quack teacher dares not pretend that there exists any *discoverable* or *proveable* anatomical or physiological *connexion* between any particular *organ* and the *result* which he assumes so confidently to exist between them. The whole system is, in a word, an arbitrary *assumption* of *all* the alleged *facts*, without the slightest pretence for any assignable or rational cause for the existence of *any one fact*.

What then is the short history of this equally comical and absurd system? It is the fabrication of a single individual, broached about half a century since, grounded not on scientific observation, or inference from proved or admitted facts. It is the boast of the craft, as it was of the founder, that it was the work of *this one man*, “he alone did it.” It is so abstract, so adverse to all common sense, so new to mankind, so incongruous with all actual, practical, and rational modes of thinking or communication of thought among men in the ordinary course of human affairs, that the inventor and his successors have been obliged to coin a *new* language to fit it; the author or fabricator of it, so vainglorious and boastful, as to approach the confines of madness on his favorite topic; the system itself, professedly resting on alleged *facts* not *reasoning*, and therefore not comprising the least degree of *science*; while the *facts* upon which it professedly rests, are of such a nature, that *certainty*, I will not say *scientific*, but merely *probable* enough, to claim the assent of any reasonable man, cannot be had; the *nature* of the facts, when any are shewn apparently consistent with the theory of the system, is such, that they prove *nothing beyond the particular instance* to which they relate; they afford no ground for general or philosophical inference, and even if thousands and tens of thousands of such facts were ascertained, yet they are inadequate to afford any useful result for the government of human

conduct, inasmuch as the admitted exceptions to any general rule are so numerous, so remote from the possibility of observation or proof to guide the inquirer, that in no possible instance can the principle or theory of the pretended science be relied on as certain or safe ; for, every general phrenological rule or maxim is confessedly liable to become inoperative when the *brain* itself is *diseased*, whether by a *latent* or an *apparent* cause ; or when there is an *organ* in the brain of a bearing or tendency opposed to or inconsistent with that of another ; or, when *education*, or *temperament*, may have *modified* or *extinguished* the natural *physiological tendency of the organ* ; or when *religion*, *morals*, or *adventitious circumstances*, suggest opposing motives to the organic suggestions—and so on interminably !

Yet this is the *system*, the science !!! which a British physician, surgeon, anatomist, physiologist, is still obtruding on the notice of a British public ! Perhaps the reader who knows Dr. E. may say, in the words of his own note, “ *this* is just the *the thing* for Elliotson” to *rave* about !! It may be so, but much more is it to be wished, that instead of raving on this topic, he would apply his talents and his professional experience to subjects on which his learning and industry might be creditable to himself, and useful to his country.

Since writing the foregoing pages, I, for the first time, met with the last edition of Dr. Combe’s work on this subject of phrenology, and desirous, if possible, before I closed these observations, to discover from it any thing which might be found there to qualify, at least, if not to remove my scepticism, my eye was attracted to a passage (p. 72, of 1st vol.) which renders me now quite assured that my hope was vain ; for, in fact, I learn from it, that *I* belong to a class of phrenological infidels whom he declines to reason with, as being without “ the reach of reason” ! In the paragraph to which I allude, the doctor inquires

“ what does the term antiphrenologist mean ?” He then enumerates three classes of opponents to his system, and the last is one under which I think I must rank, substantially, though, perhaps, not literally. He says as to this class, “ If antiphrenologist means a person who admits the mind to manifest a plurality of *faculties* by a plurality of organs, but denies that phrenologists have ascertained any of them, I ask him whether *he* disputes the *three grand propositions*: first, that dissection alone does not reveal functions ; second, that reflection on consciousness does not reveal organs ; and third, that mental manifestation may be compared with development of brain ? If he denies these principles, he is *beyond the reach of reason !*” Now, I incline to think, I rank in the disqualified class, whatever my answer may be to his *three grand principles* ; for, I certainly do admit the mind to manifest a plurality of *faculties*, and by a *plurality of organs*,—I think the mind has a *faculty* of comprehending or acquiring a knowledge of sound through the *organ of the ear* ; and I also think, it has another *faculty* of comprehending or acquiring a knowledge of ideas of *material objects* as to colours and thence of forms, through the organ of the *eye* ; and also a faculty of obtaining ideas of hardness and softness, and of roughness and smoothness, and another faculty of acquiring ideas of the qualities of heat and of cold—both through the organs of the *nerves* or *touch*. Thus, in the doctor’s words, I admit the mind to manifest a *plurality of faculties* by a *plurality of organs*, and I also deny, that the *phrenologists* have ascertained any of *them*, because *all* this learning had been obtained, and all these organs and faculties known and understood, centuries before the *science* of phrenology had arisen to amuse the brains of its disciples. But if Dr. Combe shall urge in reply, that *these common organs* are not those which he meant to allude to, but to the mysterious, though *prominent* organs, about which only phrenology is conversant, I carry my denial further to meet his meaning, and I do most devoutly deny, that the phrenologists

have ascertained or proved, to the satisfaction of any reasonable man, the existence or use of any of those organs which are properly called phrenological, in distinction from the ordinary organs to which I have alluded.

Having thus qualified myself by those admissions, to have his *three grand propositions put to me*, I am ready to answer. And as to these, my first reply is, that his three propositions are too vague to admit *precise* answers. In the first, I do not understand, precisely, what he means by *revealing functions*, nor what, by the word *functions* taken separately; if he means *physical* functions, not the *phrenological*, which always have a relation to *moral* qualities, I answer by saying, *dissection*, taken together with the knowledge which *physiology* has already ascertained with respect to these physical functions, does *reveal* such physical organs; but if by *functions*, in this first proposition, he means phrenological functions, I deny that *dissection* reveals them, for I deny their existence. As to the second grand proposition, I deny that consciousness *reveals* either *physical* or *phrenological* organs. We may, indeed, be *conscious* of the *feelings* which are the result of the operation of *physical* organs; but this cannot, in any rational sense of the word, be called a *revelation* of the organs; and as to *phrenological* organs, in *his* sense of the word, I again *deny* the existence of any such, and therefore it is I am an antiphrenologist. As to the *third* grand proposition, “that *mental manifestations* may be compared with development of brain;” I answer, that the question, whether “*mental manifestations* may be compared with *development* of brain,” is too vague to be intelligible for any purpose of his argument; for first, what does he precisely mean by “*mental manifestation*?” every indication of *thought*, of *reasoning*, of *intention* of whatever kind, is a *mental manifestation*. In this signification of the word he cannot possibly use it; if not, what is his meaning? Again, what does he mean by *development* of brain? is it *formation* of brain? is it *exposition* or *exhibition* of it? I cannot guess his mean-

ing, unless it may be simple formation of brain. Again, what does he mean by "*comparing*" manifestation with *formation* of brain? If he means by mental *manifestation* in this sentence, *thought*, the answer is, that it is *absurd*, or (if it be a more gentle phrase) *impossible* to compare a *thought* with a *corporeal* substance; for he cannot place them in juxtaposition for comparison. *Brain* is a material *substance*; *mind*, *manifestations* of mind, which must mean *thought*, these are *immaterial*. He might as reasonably ask us to compare the figure of a *square* or *circle*, with the sensation of *heat* or *cold*! The doctor and I, therefore, not speaking language mutually intelligible to each other on this SCIENCE, I must content myself with being, in the doctor's estimation, "*without the reach of reason*," and merely express my wish that the time may come, when the phrenologist and men of common sense may be able to communicate with each other in some rational hope of being mutually understood!

I have the honour to be, Gentlemen,

Your very humble Servant,

ANTI-QUACK.

ART. IX.—*A Letter from* PROFESSOR HAMILTON *of Edinburgh, in reply to the Objections made to his Practical Precepts in Midwifery by* DR. COLLINS.

TO THE EDITOR OF THE DUBLIN JOURNAL OF MEDICAL SCIENCE.

RESPECTED SIR,

In the number of your valuable publication for March, 1837, there is an article by Dr. Collins, entitled "*Observations on the Artificial Dilatation of the Mouth of the Womb, and on Instrumental Delivery, &c. &c.*" As Dr. Collins has, in that article, strongly objected to certain doctrines contained in my *Practical Observations*, then lately published,

and as I believe that the facts recorded by that gentleman himself illustrate very satisfactorily my precepts, I feel anxious to be permitted to bring those proofs under the consideration of the readers of your Journal.

It is proper that I should premise, that no one can appreciate more fully than I do, the industry and the candour with which Dr. Collins has recorded the cases detailed in his *Practical Treatise on Midwifery*, and that nothing but a sense of duty could have forced me to express my objections to many of the precepts, and to much of the practice recorded in that work.

Having volunteered to communicate to the junior members of the profession the result of my experience in the treatment of the process of human parturition, and being impressed with the solemn belief that the rules I have suggested are well founded, I consider myself bound to defend those doctrines, whenever they are impugned by practitioners of undoubted respectability. Upon this principle I have already, in certain letters addressed to the Editor of the *London Medical Gazette*, noticed the article alluded to in your Journal for March, 1837, and I have, I trust, proved, that many of Dr. Collins's animadversions proceeded from a misunderstanding of my meaning. But my present object is to shew to the members of the profession in Ireland, who have been my pupils, that Dr. Collins's own recorded cases fully establish the utility and importance of those practical precepts which I have so long taught, and which have been so much condemned by him.

MANAGEMENT OF THE FIRST STAGE OF LABOUR.

One of the earliest innovations in the treatment of human parturition, which I found good reason to introduce, was the limiting the duration of the first stage of labour to twelve or fourteen hours, whenever the uterine contractions continue to be regular and progressive; and I have stated in my *Practical Observations*, that the following are the necessary effects of the protraction of that process beyond the time specified.

Firstly. The powers of the uterus may, in the second stage, be inadequate to the expulsion of the infant, with safety to its life, or to the future health of the mother.

Secondly. After the birth of the infant, the uterus may contract irregularly, so as to occasion the retention of the placenta.

Thirdly. After the expulsion of the placenta, the contractions of the uterus may be too feeble to prevent alarming hæmorrhage.

Lastly. Supposing the patient to escape all those untoward circumstances, febrile or inflammatory affections of a most dangerous nature may ensue from the previous protraction of pain, and irregular distribution of the blood.

In other words, my conviction is, that although there be no injurious pressure upon the person of the infant, nor on that of the parent, the continuance of pain from uterine contractions, above a certain number of hours, must occasion more or less exhaustion, both of the sensorial and of the muscular powers, and must necessarily influence the subsequent process of delivery.

I have added, (*Practical Observations, Part First, page 196,*) that by the adoption of this rule, I can confidently assert, that no patient under my charge, for the last thirty-five years, has been above twenty-four hours in labour, and, excepting in cases of disproportion, none so long.

Against this practice, Dr. Collins has objected in very strong language. He says, page 40 of your Journal : “ I have, however, studied every page with the closest attention, nor can I find fact, or argument, to induce me to alter my decided opinion, that such measures, (alluding to the means suggested for limiting the first stage to twelve or fourteen hours,) thus warmly urged, are not only uncalled for as far as regards the safety of the patient, but if generally acted upon, likely to be followed by serious results both to the mother and child.”

This anathema is very inconsistent with Dr. Collins's usual candour, and is certainly not a little startling. I have stated strongly the fact, that no patient under my care for thirty-five years, previous to my publication, had been above twenty-four hours in labour, and excepting in cases of disproportion, none so long. If Dr. Collins deny this assertion, I shall refer him at once to the testimony of public opinion in this city; and that it is a fact bearing strongly upon the question at issue, the cases of protracted labour in the Dublin Lying-in Hospital must establish to the conviction of the most prejudiced minds.

For the correctness of the arguments in favour of the practical precept, I confidently appeal to the following cases published by Dr. Collins himself:—

(A) Page 465, No. 210. “A woman, of a most fretful and anxious disposition, was admitted February 17th, to be confined of her first child.

“On the night of the 18th she complained of pain and uneasiness, which she supposed was her labour, yet there was no dilatation of the os uteri; the next day she still complained of some uneasiness, but slept the entire of that night.

“On the morning of the 20th the pain and uneasiness returned in a more urgent form, and she expressed herself as suffering the most acute distress; still there was no dilatation of the mouth of the womb, it was quite thin and lax, and the head was low in the pelvis; the waters had been dribbling away from the time she was admitted. The pains continued during the night. The following morning at 9 A. M., the os uteri was dilated to the size of half a crown, but the pains had not assumed a bearing down character. She had frequent pains during the day, and the succeeding night had intervals of ease; but slept little.

“On the 22nd the pains still continued, yet the head made no progress, and the mouth of the womb was very little more dilated.

“From this time till the following morning, the 23rd, the

uterus continued to act imperfectly ; the labour notwithstanding made very considerable progress, the os uteri being now tolerably well dilated, except towards the pubes, where it still covered the head of the child. The pelvis felt of sufficient size to allow the head to pass, and all that seemed wanting to effect this, was, that the pains should become expulsive. The bowels from the commencement had been attended to with much care, and the abdomen was quite free from pain on pressure. The pulse after this became hurried, breathing difficult, accompanied with great anxiety and considerable debility. It was now thought advisable to administer an opiate to procure rest, in the hope that the uterus would act with more effect afterwards ; thirty-five drops of tincture of opium, with three drachms of castor oil, were given, followed by quiet rest till the evening. At 8 P. M. she was easy, had little or no labour pain, and took some gruel. An hour afterwards, she was seized with the greatest difficulty of breathing, amounting almost to suffocation, accompanied by considerable debility ; the pulse was scarcely to be felt, and the extremities cold. On examination the head was found in the same situation as in the morning, and had it not been that the mouth of the womb still remained over it, next the pubes, an attempt would have been made to deliver with the forceps. The head was immediately lessened, and almost every bone removed before it could be delivered ; and even after it was brought down, much exertion was required to free the shoulders and body. The child was large, and the abdomen somewhat distended with air.

“ The mother seemed at this time almost lifeless, having lost the power of swallowing. The hand was introduced into the uterus, which was quite relaxed ; the placenta was gently removed, and the patient expired immediately.

“ On dissection, the uterus was found healthy, but badly contracted, containing a small quantity of coagulated blood ; the intestines were in the highest state of congestion, and there was about a pint of fluid in the abdominal cavity, with portions

of coagulable lymph in different parts, seemingly the effects of inflammation previous to labour. On opening the chest, the lungs were observed to adhere so firmly as to require the knife in many places to separate them. Nothing was discovered to account for the suddenness of death."

In this case it is admitted, that on the 20th February, labour begun by the discharge of the liquor amnii, the mouth of the womb being quite thin and lax, and the head low in the pelvis; that on the morning of the 23rd, the os uteri still covered the head of the child, while the pelvis felt of sufficient size to allow the head to pass; that instead of assisting the dilatation of the os uteri, an opiate was given; that at eight of the evening of the following day, most alarming symptoms occurred, requiring instant delivery; immediately after which the patient expired; and Dr. Collins expresses himself surprised at the suddenness of death. Mr. Travers, who does not practise midwifery, would have told him, "that it was occasioned by the protraction of pain."

As this article is professedly written for the junior practitioners of Ireland, I beg leave to state the practice which I should have recommended in the above case, which will supersede the necessity of minute details on some of the other cases.

When the membranes burst, and the head was low in the pelvis, and the mouth of the womb quite lax and thin, I should have advised pressure upon the edges of the os uteri with the points of two fingers during every pain,* and if, when that orifice was fully dilated, which I have no doubt might have been accomplished within a short time, (certainly within two hours,) there had been no expulsive pains, I should have applied the forceps and extracted the infant. In the above instance, there-

* "In the case I have just considered, I have spoken of the effects of dilating the os uteri; but I do not mean to say that the practice is useful in such a one alone, for in most cases of tedious labour it is beneficial. Two circumstances are necessary to render it safe: the os uteri ought to be already very considerably opened; its edges must be lax, dilatable, and, generally speaking, thin."—*BURN'S Principles of Midwifery*, 9th edit. p. 446.

fore, it must appear to any candid inquirer after truth, that both mother and child were victims to the practice adopted: the cause of protraction was evidently the interception of a band of the cervix uteri between the head of the infant and the pubes.

The following case shows the injury to the infant from the protraction of labour, where the mother was safely delivered, though there may be good reasons to question whether her future health may not have been endangered by the protraction.

(B) Page 471, No. 608. "The labour pains were very tardy and feeble, producing much irritation, without causing any dilatation of the mouth of the womb. In this state she remained for thirty hours; after which opiates were given three times at considerable intervals, each time with benefit, and at the expiration of fifty-three hours, she was delivered naturally of a still-born child."

It is much to be regretted that the full details of this case are not given.

A further illustration is furnished by the following case, of the injurious effects of the protraction of the first stage.

(C) Page 475, No. 725. "This patient, when admitted, was reported to have been sixty hours in labour; the os uteri was very little dilated, and the head high up in the pelvis. The pains continued constant for twenty-four hours after she came in, yet the labour made little progress; the mouth of the womb was rigid, jagged, and had the feel of cartilage. The child being dead, as indicated by the stethoscope, the head was lessened, and left in that state for some hours, and afterwards cautiously brought down. Considerable force was necessary to complete the delivery, though the child was putrid."

This woman had been, before admission into the hospital, sixty hours in labour, the pains continued for twenty-four hours after she came in; the mouth of the womb was rigid, jagged, and had the feel of cartilage, (which clearly explains the cause of difficulty,) and yet, no means were employed for promoting its dilatation.

Had no other case occurred in the Dublin Lying-in Hospital than the above, the injurious consequences of allowing the first stage of labour to go on to an indefinite extent, ought to have roused the attention of the medical officers of that noble institution.

But, the following case is so remarkable in all its circumstances, that it ought to have made a very different impression on the minds of the gentlemen alluded to, than it seems to have done.

(D) Page 481, No. 1038. “ This patient was admitted in labour of her first child. Uterine action was feeble, and continued so for seventy-two hours after she came in. As the foetal heart had ceased to act for some time, and the pulse became hurried, it was considered advisable to deliver her. The os uteri was not fully dilated, the head was high and resting on the pubes ; it was lessened, and cautiously brought down with the crotchet.

“ Severe abdominal inflammation set in shortly after delivery, which resisted the most prompt and active treatment, and proved fatal on the 6th day.

“ On dissection, the intestines were found matted together, with an extensive deposition of lymph on their surface ; there was also some sero-purulent fluid in the cavity of the abdomen. The uterus was coated externally with greenish lymph, and, on its internal surface, there was a coating somewhat similar in appearance. There was a very small opening in the lip of the uterus so as to admit the point of the finger, and a second similar one in the vagina, about half an inch below the mouth of the womb.

“ The pelvis measured three and a half inches from pubes to sacrum, and four and a half transversely.”

That the first stage in this melancholy case was allowed to go on for seventy-two hours is candidly admitted. Whereas, if the os uteri had been supported within a proper time, and the other resources for facilitating its dilatation had been employ-

ed, the probability is, that both mother and child might have been saved.

(E) Page 142, No. 110. “ This woman was sixty hours in labour ; shortly after the birth of the child there was considerable hæmorrhage, with hour-glass contraction which was treated accordingly. The child died thirty hours after birth.”

(F) Page 169, No. 130. “ In this case the labour lasted fifty hours, the foetal heart having ceased to pulsate, and the head having made no progress for several hours, the mother’s pulse being 120, the head was lessened, and delivery effected by the crotchet. The placenta was thrown off in half an hour, followed immediately by considerable hæmorrhage. Increased pressure was made and cold applied ; but in five minutes it returned to a serious extent, when the hand was passed, and the uterus, being emptied of its contents, contracted well, and the discharge ceased. She got then thirty-five drops of tincture of opium to procure rest.”

(G) Page 169, No. 129. “ This woman was sixty hours in labour ; immediately after delivery, severe hæmorrhage ensued, which was checked by the introduction of the hand ; after which she got forty drops of tincture of opium.”

The above seven cases, I flatter myself, furnish the most decided proofs of the injurious consequences of the indefinite protraction of the first stage of labour, and of the validity of the arguments upon which I originally founded the rule of limiting the duration of that stage to twelve, or fourteen hours.

With no little surprise did I read, after the strong expressions in page 40 of your Journal, in 58 and 59 of the same Journal the following paragraphs :

“ I have not entered into any details of the measures recommended by Professor Hamilton to effect the dilatation of the os uteri within a limited period, as, disbelieving in the utility of the measures, it is unnecessary. For information on these points, the work itself may be consulted.”

“ In some instances, especially with first children, the

mouth of the womb continues rigid and hot, with little tendency to yield under uterine action, accompanied not unfrequently with considerable irritation. In such, bleeding to the extent of ten or twelve ounces, and keeping the patient under the influence of slightly nauseating doses of tartar emetic, (to which a small quantity of opium should be added,) will be found to promote relaxation, and thus, be productive of the best effects. In others, where a fold of the os uteri continues to be forced down before the head anteriorly, between it and the pubes, although elsewhere obliterated, the descent of the head will be much facilitated by applying two fingers, so as to keep it stationary during the pain, and thus permitting the head to clear this obstruction. Neither of these cases are often met with, nor have they any tendency to illustrate the opinions noticed. I make the observation here, having had practical experience of the advantage of the treatment."

From the preceding detail, it is evident, that when the cases which have been recorded by Dr. Collins, where the patients had been allowed to have been, p. 169, No. 130, fifty hours; p. 471, No. 608, fifty-three hours; pp. 142 and 169, Nos. 110 and 129, sixty hours; p. 481, No. 1038, seventy-two hours; p. 475, No. 725, eighty-four hours; and p. 465, No. 210, ninety-six hours in labour, in consequence of the protraction of the first stage, the doctor had not been aware, that bleeding, nauseating doses of tartar emetic, with opiates, administered during the first stage, promote relaxation, and are thus productive of the best effects, and also, that he had not then from experience learned, that the descent of the head may be much facilitated where there is a fold of the os uteri forced down before it anteriorly, by applying two fingers, so as to keep it stationary during the pain.

What must be the impression of every reader when he thus finds, that Dr. Collins declares, after having in such decided language objected to my advice, to limit the duration of the first stage of labour to twelve or fourteen hours, that, from ex-

perience he has found, that the means recommended by me are “productive of the best effects.”*

Had not Dr. Collins shown so much candour throughout the whole of his *Practical Treatise on Midwifery*, it might be supposed that the reason for not having specified (in the above paragraph) the measures I recommend for securing the completion of the first stage of labour within twelve or fourteen hours, was a consciousness that the intelligent reader would have at once discovered that he had adopted (though not while Master of the Dublin Lying-in Hospital) the very practice, of the utility of which he still expresses his disbelief.

But on this point I offer no opinion, though certainly the following paragraph does not tend to elucidate the subject. He says in page 43 of your Journal, in reference to his practice in the Dublin Lying-in Hospital, “In no single instance in all these cases (15,850) were any means whatever used to effect the dilatation of the mouth of the womb within any given period; nor was artificial assistance ever attempted until the safety of the patient *absolutely required* it.”

On this subject I quote, with great pleasure, the words of Professor Burns of Glasgow, 9th edition, page 448: “If the pains be continuing without suspension, or an interval of some hours, and the labour being going on all the time, but slowly, it is a good general rule to effect the dilatation of the os uteri, within ten or twelve hours from the commencement of regular labour.

ON THE TREATMENT OF LABORIOUS LABOURS.

A second part of Dr. Collins’s objections to my doctrines, relates to the practice in cases of laborious labours. My Prac-

* Those measures are venesection, opiates, and supporting the os uteri during the pain, according to the several causes of protraction, and it is to be specially noticed, that I have stated that although spontaneous vomiting speedily promotes the dilatation in certain cases, I have not ventured to recommend artificial emetics, having found that the other means are both safe and efficacious.

tical Precepts on this subject may be briefly stated in the following summary.

Firstly. That it is in the power of the practitioner to judge so opportunely whether the labour pains tend to advance the delivery, as to prevent the occurrence of injury either to the infant, or to the mother.

Secondly. That after the second stage has commenced, if regular pains continue, and the infant becomes wedged in the passage, the practitioner is imperatively called upon (supposing the infant within reach of the forceps) to interfere before there be a probability that the pressure of the uterus upon the navel string may prove fatal to the infant, and certainly, before any untoward symptoms threaten the immediate, or eventual safety of the mother.

Thirdly. That the forceps, properly applied, can do no harm whatever to the mother, while, by diminishing the bulk of the infant, it enables the practitioner to lessen, as well as to shorten her sufferings.

Fourthly. That when called to a case of protracted labour, which had been previously mismanaged, the state of the mother is to be principally, but not exclusively considered, and if immediate delivery be required, the forceps or the crotchet should be had recourse to, without regard to the state of the infant. In other words, that if the forceps can be applied with safety to the parent, that instrument is to be used ; but, if the previous pressure on the linings of the pelvis have excited tenderness or swelling, then the crotchet is to be preferred, even although the infant be alive, upon the established principle, that the safety of the parent should be the first object of the practitioner.

I shall now detail *seriatim* the proofs in support of those propositions afforded by Dr. Collins's recorded cases.

Firstly. (H) Page 158, No. 126. " This woman was fifty-nine hours in labour ; it was her first child, the pains were, for a considerable time, very trifling, with long intervals ; however, for the last twenty-four hours, the uterus acted with tolerable

regularity, the pains being at times strong, causing the head to press with much force against the ischia, where it remained stationary for the greater part of that time. Her pulse was very much increased in frequency, varying between 120 and 130; the external parts were œdematous. As the foetal heart had ceased to act (having been distinctly audible in the right iliac region six hours before,) the head was lessened and the crotchet applied. The placenta was expelled in forty-five minutes, immediately after which, in consequence of hæmorrhage, the hand was introduced, and so it was arrested.

“Violent inflammation and sloughing set in, resisting all treatment; and she died on the ninth day. For four days previous she had severe diarrhœa, a succession of motions coming on suddenly, with extreme pain; she had also severe hiccough. On examination after death, the vagina was found in a state of slough; the sides opposite the spines of the ischia were broken through with the slightest force, and were completely gangrenous; a circular opening, the size of a shilling, was found forming a communication between this cavity and the rectum, the mucous surface of which, as also that of the colon, was softened, and had, in the vicinity of the opening, a gangrenous appearance. There was no symptom of inflammation in the peritoneum or uterus.”

In this case, as the patient was in the hospital from the beginning of labour, we are bound to believe that she had been properly treated from the commencement; that “strict attention had been paid to keep her cool, and her mind easy;” that “stimulants of all kinds had been prohibited;” that “the state of her bowels and bladder had been duly regulated;” and that the symptoms had been watched by “diligent and persevering attendance at the patient’s bed-side.”—*Practical Treatise on Midwifery*, page 16. It may, therefore, be asked, upon what principle this woman was allowed to suffer for so many hours with the pulse between 120 and 130, while the head of the infant was pressing so injuriously upon the ischia as to produce

actual gangrene of the vagina, and œdematous swelling of the external parts ?

Had the circumstances narrated in this case occurred in England, in a parish workhouse, the conduct of the medical attendants would undoubtedly have given rise to subsequent inquiry.

Dr. Collins has well observed, "that there is no subject connected with the practice of midwifery so difficult to acquire a sound knowledge of as the treatment of tedious and difficult labours. It is one of the most vital importance, and in the most marked manner distinguishes the experienced from the inexperienced practitioner."—*Practical Treatise*, p. 7.

The reader will judge from the following cases whether the practice pursued in the Dublin Lying-in Hospital ought to be adopted, or whether the rule which I have laid down should be followed.

(I) Page 207, No. 21.—"A. B., after having been nearly forty-eight hours in labour, was suddenly attacked with convulsions, for which she was bled, to the extent of twenty ounces, with relief ; yet the fits returned twice afterwards with violence. The pains from the commencement had been tardy and inefficient ; for the last twenty hours the head had made but little progress, still it advanced slightly, and was pressing on the perineum : it was so firmly impacted in the pelvis, and the pressure on the urethra was so great, as to render the introduction of the smallest-sized catheter into the bladder impracticable, which was at the same time distended with urine : her pulse was feeble and hurried, 136, and her strength much exhausted : the head was immediately lessened, and the child brought away by the crotchet. The placenta was expelled immediately afterwards, when she fell into a sound sleep ; out of which, in about three quarters of an hour, she awoke in a severe convulsive paroxysm. She was now given forty drops of tincture of opium, which induced sleep, and she had no return of the attack.

"Abdominal inflammation set in next day, which, notwith-

standing most decided treatment, proved fatal on the third day. Her friends would not suffer the body to be examined."

If the attentions recommended by Dr. Collins had been faithfully observed in this melancholy case, it is impossible to believe, that the head of the infant could have been allowed to be, for many hours, so firmly impacted in the pelvis as to render the introduction of the catheter impracticable, with a pulse at 136, feeble and hurried, with the strength much exhausted, and that, too, after the patient had had a fit of convulsions.

(J) Case 462, No. 49. "Was forty-eight hours in labour of her first child. Having made no progress for the last twenty-four hours, the pulse becoming extremely quick, with great general debility, the head was lessened, and delivery effected with the crotchet. Considerable difficulty was experienced in getting the head through the pelvis, in consequence of the hand having descended with it."

An attentive practitioner could have certainly discovered that the hand of the infant had descended with its head, long before the lapse of twenty-four hours, and *long before* the pulse of the patient *had become extremely quick, with great general debility*, and certainly before the pressure had destroyed the infant.

(K) Page 464, No. 150. "Was forty-eight hours in labour in the hospital, the waters having been discharged a considerable time before admission. For several hours after she came in, the labour pains were neither severe nor frequent; however, the uterus afterwards acted well, and the head was forced so low as to cause the scalp nearly to protrude, when it remained stationary for twelve hours. The ear could be distinctly felt next the pubes, and there was sufficient room towards the sacrum to admit the introduction of the forceps with ease, yet, in the transverse direction of the outlet, there was evidently a diminution in size. It was thought, however, as the head was so low, by gentle assistance it might be got down; no force, notwithstanding, consistent with safety, was found sufficient. As

the patient's strength was rapidly sinking, and the abdomen had become tender on pressure, delivery was accomplished by lessening the head."

That any practitioner "diligently and perseveringly attending at the bedside of the patient," could allow the scalp to be protruding through the external parts for twelve hours, and sit by without offering any assistance, while the patient's strength was rapidly sinking, and the abdomen had become tender on pressure, must appear to every unprejudiced person to be quite inexcusable.

(L) Page 470, No. 509. "Was thirty-three hours in labour of her third child, without having made the least progress for the last twelve. The bladder was forced down before the head. Her pulse became much hurried, and strength greatly exhausted, rendering immediate delivery necessary. The head was lessened, and the child brought away by the crotchet. It was very large. All her former children were still-born."

It is sufficient to point out to the reader that this woman, who had already been delivered of two still-born infants in the hospital, was allowed to be thirty-three hours in labour, the child not having made the least progress for twelve hours, and her pulse having become much hurried, and her strength greatly exhausted, before the proper assistance was afforded.

(M) Page 470, No. 526. "Was reported to have been twenty-four hours in labour before admission. About twelve hours after she came in, it was discovered, that the face was turned towards the pubes, and pressing so strongly on the urethra, that the catheter could with difficulty be passed. The pains continued strong for fifteen hours from this time, yet the head did not advance. It was deemed advisable to lessen it.

"This patient had been in the hospital thirteen months previously, and was then delivered with the crotchet, of her first child, after a labour of three days. She was brought from the county Meath."

The facts admitted in the above case are : "that the woman

was brought into the hospital after having been twenty-four hours in labour ; that it was not till twelve hours after admission that it was discovered that the face had turned under the pubes ; and that fifteen hours were allowed to elapse after this discovery before means were adopted to relieve the patient, notwithstanding strong pains, without any advance of the infant, and strong pressure upon the urethra.

(N) Page 464, No. 173. “ Was delivered with the crotchet after sixty-four hours’ labour, having made no progress for the last twenty-four : the child was evidently dead, and the pressure on the urethra was very severe ; when brought away, it was found large and putrid. This woman died on the thirteenth day after delivery. On dissection, a stricture of the intestine was found immediately above the sigmoid flexure of the colon ; several adhesions were observed between the liver and colon, apparently of old standing. In both cavities of the thorax, extensive effusion had taken place, with a considerable deposition of lymph : the lungs were firmly adherent. The heart was extremely large, and gorged with blood ; its parietes were thickened. The uterus was perfectly healthy, and well contracted : the pelvis was considerably diminished in size, in consequence of a projection of the last lumbar vertebra.”

Instead of commenting on the above cases, I shall quote the words of Dr. Collins himself :—

“ I know of no case where the advantage derived from the use of the stethoscope is more fully demonstrated than in the information it enables us to arrive at, with regard to the life or death of the foetus, in the progress of tedious and difficult labours.

“ Heretofore we were in a great measure ignorant of the time, at which, death took place ; and the practitioner imagining the child alive, from want of satisfactory evidence of its death, delayed interfering, until his patient was in the greatest possible danger ; whereas, had he been assured the child was dead, he would have delivered her before life became actually hazarded,

and thus prevented her not only enduring for hours, but even days, in some instances, the most torturing pain ; the result of which continued suffering was not unfrequently death, or which was perhaps worse than death, extensive sloughing of the urethra, or of the recto-vaginal septum, establishing a communication between these two cavities, reducing the unfortunate sufferer to a state of extreme misery."—*Practical Treatise*, p. 18.

Notwithstanding the excellent maxim thus quoted, it is evident that the symptoms indicating the approach of danger had been totally overlooked in the above cases, and that they establish, beyond a possibility of contradiction, my first proposition ; for, in the cases alluded to, inattention to that rule proved fatal to the mother and to the child.

Secondly. Dr. Collins states, that in 16,414 women delivered in the Dublin Lying-in Hospital during his mastership, there were twenty-four forceps cases ; that four of the women died, and that eight of the infants were still-born : in plain language, that one-sixth of the women and one-third of the infants died ; and yet the great object, in the application of the forceps, is to save both mother and child. Let this result be compared with that of the practice adopted in the Hospice de la Maternité of Paris.

According to Baudelocque, during nine years, ending 1806, there were 12,751 deliveries in the Hospice de la Maternité. Out of these, the forceps cases were thirty-seven, being one in $344\frac{2}{3}$; and the crotchet cases were nine, being one in $1416\frac{2}{3}$.

Madame la Chapelle has given an account of the result of the practice in the same hospital, preceding the 31st December, 1811. She states, that in 15,652 deliveries in that hospital, there were ninety-three forceps cases, and seventy-two of the infants were born alive. In the details given by Madame la Chapelle, she acknowledges only the death of thirteen women, which is one-seventh instead of one-sixth which occurred in the Dublin Lying-in Hospital.

But this is not the only fact which requires the most serious

attention. In the Dublin Lying-in Hospital, according to Dr. Collins's records, the forceps were employed only once in 608 cases ; whereas in the great hospital at Paris, according to Baudelocque, it was used once in $344\frac{2}{3}$; and according to Madame la Chapelle, in the same hospital, once in 165 cases. In the Edinburgh General Lying-in Hospital, during a very limited period, the forceps cases were one in 109.

On the other hand, in the Dublin Lying-in Hospital, the crotchet was used in laborious labours, according to Dr. Collins, once in 210 cases.* Whereas in the Hospice de la Maternité, according to Baudelocque, crotchet cases bore the proportion of one in 1416 ; and according to Madame la Chapelle, one in 1908 ; while in the Edinburgh General Lying-in Hospital, the proportion was one in 481. The reader will now find no difficulty in deciding to whose practice the accusation (Dr. Collins, Dublin Journal, p. 40) of "cruelly destroying the child" is applicable.†

The following cases are quoted in illustration of my second practical precept.

(O) Page 473, No. 665. "Was thirty-five hours in labour of her first child, for the last twenty-four of which, the head had not made the least progress. Her strength being ex-

* The number of crotchet cases in laborious labours was seventy-nine, but that instrument was had recourse to in other cases of difficulty ; so that during the seven years of Dr. Collins's incumbency, there were 118 crotchet cases, bearing the proportion of one to 141.

† In illustration of this important precept, I referred, in the second part of my Practical Observations, p. 100, to several of Dr. Collins's recorded cases, and I find that, in so doing, I had committed a gross error, for which the most ample apology is due to Dr. Collins. While I regret this error, which was most unintentional, I think that I can explain it. In the course of reading his work, I made a memorandum of all the cases in which it appeared to me that there had been an injurious delay in affording the necessary assistance, and had afterwards selected the cases where there had been disproportion, without having marked off those latter cases from the general memorandum ; unluckily, therefore, both lists were printed, the original one, page 100, and the selected one, page 162.

hausted, and the child being some hours dead, as ascertained by the stethoscope, delivery was effected by lessening the head.

“She continued to recover favourably till the fourth day after delivery, when she was suddenly attacked with the most acute pain in the abdomen, which resisted the most active treatment, and she died in forty-eight hours.

“On dissection, a large quantity of a deep, straw-coloured fluid was found in the abdominal cavity, and all the viscera were extremely vascular. The uterus was soft, but in other respects healthy; the vagina was in a sloughing state.”

A case more clearly demonstrative of the necessity of applying the forceps in due time could not have been fabricated; than the true history of this case unequivocally proves. No disproportion between the mother and child is even alleged, and in point of fact, by the appearances on dissection, it was ascertained that none existed, and yet the infant's head was allowed to remain so long wedged in the passage, as to occasion sloughing of the vagina.

(P) Page 303, No. 10. “Was admitted on the evening of the 16th February, in labour of her fifth child. The uterine action was tardy, the head made little progress, and the waters were discharged early the following morning. The pains continued at intervals during the day, but in the course of the succeeding night, became more forcible. She slept occasionally during their absence. Suddenly, at five, A. M., on the 18th, all uterine action ceased, immediately followed by great debility and vomiting. An effort was now made to deliver her with the forceps; which being found impracticable, the crotchet was substituted. After the placenta came away, an extensive laceration was found, towards the pubes, at the junction of the uterus and vagina; the peritoneal covering was safe. She expired, almost before the delivery was completed.

“This woman had complained of a severe crampish pain in her right thigh, for a considerable time before delivery.”

It appears to me, that in this case the forceps ought to have been applied long before the period at which the rupture of the uterus took place.

Thirdly. My third proposition relating to the utility of the forceps, as lessening both the duration and the sufferings in laborious labours, is fully established, according to my opinion, by the following cases recorded by Dr. Collins.

(Q) Page 469, No. 425. “Was fifty-eight hours in labour, for the last twenty-four hours of which, the head made no progress, although the pains were strong during the greater part of that time. As the ear was within reach of the finger, the forceps were introduced, but no force, consistent with safety, was of the least service. The head was then lessened, and delivery accomplished with the crotchet.”

(R) Page 472, No. 639.—“Was forty-eight hours in labour ; it was her sixth child ; all the former were born alive. The head for twelve hours previous to delivery made no progress, although the uterine action was at times so violent, as to lead us to dread rupture. She complained of most acute pain in her right leg and thigh ; and her pulse became hurried : the soft parts were well dilated, yet the ear could with difficulty be reached with the finger. The forceps were cautiously introduced ; and considerable exertion was required to effect the delivery, the child being unusually large. It was still-born, though the heart’s action was audible a short time previous.

“Immediately on the birth of the child, most profuse hæmorrhage set in, requiring the instant introduction of the hand for the placenta, the greater part of which was found in the vagina : on its removal, the discharge ceased, and by careful binding with compress, and the use of cold applications, there was no return.

“She was delivered on the 13th of February : on the 15th she complained of tenderness of the abdomen, which was removed by leeches and stuping : on the sixteenth she suffered from uneasiness in her stomach : and on the morning of the

17th her pulse sunk rapidly, and her extremities exhibited, in the most marked manner, the appearances of diffuse cellular inflammation, particularly the right fore-arm. Her strength continued to fail; and she died the same evening, although stimulants and cordials were diligently employed.

“ On dissection, the abdominal viscera appeared healthy. There was a slight blush of redness on the anterior surface of the uterus. The muscles of the body were in a remarkable state of decomposition, particularly those of the right fore-arm, where they appeared in a state of putrefaction. The blood was fluid in all parts of the body.”

(S) Page 475, No. 674. “ This patient was thirty-six hours in labour, (first child,) the head not having advanced for the last twelve. The parts being well dilated, and the ear within reach, delivery was accomplished with the forceps. The child was still-born.”

On these cases I have no other remarks to offer than what I have already stated in my *Practical Observations*, Part II., page 112. That Dr. Collins had used forceps, ill adapted to laborious cases, is evident from his own admission; for he says, “ that the blades of the smallest sized British forceps, with which he was acquainted, when completely closed, measured from $3\frac{1}{8}$ to $3\frac{1}{2}$ inches asunder;” whereas the forceps which I have always used and recommended, measure at least one-eighth under three inches; and I have asserted in my letter to the Editor of the *London Medical Gazette*, dated August 19th, 1837, page 753, that within these twelve months I extracted a living infant, with perfect safety to it and the mother, the long circumference of whose head measured $17\frac{1}{2}$ inches, as witnessed by two respectable practitioners of this city, to whose testimony I can confidently appeal. It will be observed, that the largest circumference of the infant's head described by Dr. Collins (*Practical Treatise*, page 12) is fifteen inches.

One remarkable feature of this case, which cannot fail to strike intelligent practitioners, is, that the infant had discharged

its fæces, while the great size of the head, having filled completely the pelvis, prevented that circumstance being known ; and therefore, if the infant had remained a few minutes longer unborn, its death was inevitable ; and that the application of the forceps within two hours and a quarter after the uterine contractions had ceased to advance the infant, was prompted by a conviction that further delay might be injurious both to mother and child.

Fourthly. The precept, that in cases of laborious labours, which had been mismanaged, the practitioner called in is to attend principally, though not exclusively, to the state of the mother, is very different from the opinion of Dr. Collins. He says : “ the death of the child takes place in laborious and difficult labour, before the symptoms become so alarming as to cause any experienced physician to lessen the head.”—*Practical Observations*, page 16.

The attentive reader will have little difficulty in ascertaining how far the following cases, detailed by Dr. Collins himself, warrant such an axiom.

(T) Page 300, No. 32.—“ This patient was sent many miles from the country to hospital, in severe labour, on the evening of the 27th May. On admission, her countenance was expressive of great anxiety ; her pulse 120 ; the foetal heart acting with rapidity ; the head low, and fixed in the pelvis. Delivery was effected the next morning at nine o’clock, by lessening the head, as the child’s heart had ceased to beat ; and the patient had become extremely feeble, having vomited several times a dark brown fluid : a rupture was suspected from the symptoms present.

“ After delivery, her strength continued rapidly to fail ; the abdomen became distended and tympanitic ; and she died in fourteen hours.

“ On dissection, there was evidence of extensive peritoneal inflammation ; the uterus was thrown very much to the right side, and, at the left, its muscular substance was found to have

given way, close to the vagina. The peritoneum was not injured, but was raised up and distended with blood underneath, resembling a bladder."

This case affords a most complete contradiction to Dr. Collins's opinion. The woman, when admitted into the hospital in severe labour, had a countenance expressive of great anxiety, with a pulse at 120, and was evidently in imminent danger; and yet the foetal heart was acting with great rapidity.

(U) Page 483, No. 1091.—"Was admitted August 23rd, in labour of her first child, and was not delivered until the 25th, being a period of fifty-six hours. Uterine action from the commencement, until within six hours of the expulsion of the child, was extremely feeble, with long intervals. The head remained high in the pelvis, and although the ear could not be reached, it was evident the head had sufficient room to pass; to effect which, uterine action was alone wanting. As soon as the pains began to be brisk, the labour proceeded without difficulty. The foetal heart was quite audible until eight hours previous to the birth.

"In three hours after, the hand was passed to remove the placenta; it was found separated, and without the slightest effort, the uterus contracted and expelled both. The perinaeum had suffered considerably in the passage of the head.

"This patient never seemed to rally after delivery; the pulse continued quick; there was considerable tenderness, on pressure, over the uterus, with a foul discharge from the vagina. She was treated with small quantities of calomel until the mouth became affected, which produced debility, relieved by mild aperients. She was put on nutritious diet when the abdominal distress had subsided, which occurred when the mercury affected the system. On the seventh and eighth days she had distinct rigors, followed by perspirations; after which her strength became greatly reduced. The vaginal discharges continued foul, notwithstanding the most rigid attention to cleanliness, and the use of stimulating injections. She gradually

sunk, and died on the eleventh day, having, for two days previous, suffered from frequent hiccough. On dissection, the only morbid appearances found were in the bladder and vagina. In the bladder, the mucous surface was covered with yellow lymph, and it contained a quantity of muco-purulent fluid. In the vagina, opposite the right ischium, a portion appeared to have been destroyed by slough, but its texture did not, in other parts, seem materially injured, although of a darker colour than natural.

“ This was a very singular case, as there was nothing apparently in the labour in any way calculated to induce such an unfavourable termination.

“ She was a feeble, emaciated woman, and seemed to have suffered from hardship.”

Many comments might be made upon this case, but it is quoted to prove that Dr. Collins's allegation “ that the death of the child takes place in laborious and difficult labour, before the symptoms become so alarming, as to cause any experienced physician to lessen the head,” is unsupported by his own facts, and is, as I hold, a most dangerous opinion. He admits, that this woman was forty-eight hours in labour, before the infant's death was supposed to be ascertained, during which time, the head remained high in the pelvis, that six hours previous to delivery the uterus acted well, and the child was expelled by the natural powers, and, in his account of the appearances on dissection, he says, that “ the morbid appearances were in the bladder and vagina,” that “ in the bladder the mucous surface was covered with yellow lymph,” &c. No one can doubt, that the injury done to the bladder and vagina was the effect of the head having remained high in the pelvis for forty-eight hours, and there can be as little doubt, that the death of the patient was principally owing to the protraction of pain.

I may take the liberty to add, that if the pupils of the Dublin Lying-in Hospital are taught to wait three hours after the birth of the child before extracting the placenta, they will, if

they follow such a rule, meet with many distressing cases in the course of their practice.

Other cases may be quoted to prove, that several women's lives in the Dublin Lying-in Hospital, in whom the labour was protracted, were brought into great jeopardy, while the child, according to the evidence of the stethoscope, continued to live.

By what extraordinary process of reasoning any practitioner, with the conviction that there is such an obstacle to the delivery, that a living infant cannot be born, should delay the necessary relief to the woman, till by means of the stethoscope the death of the infant should be ascertained, is to me quite incomprehensible.

And yet the following cases show, that this principle was carried still further in the Dublin Lying-in Hospital, for it is admitted, that notwithstanding the alarming symptoms, the sufferings of the patient were allowed to continue for hours after the death of the infant was believed to be unequivocally ascertained by means of the new method, on the important utility of which Dr. Collins has passed so high an eulogium.

(V) Page 471, No. 555. "Was sixty hours in labour of her first child. The pelvis was defective, and there had been no advance for the last twelve hours; the child's death having been ascertained by the stethoscope some hours previous, the head was lessened, and delivery thus completed."

(W) Page 471, No. 584. "Was thirty-six hours in labour of her first child, and as its death had been ascertained by the stethoscope some hours before, delivery was accomplished by lessening the head."

(X) Page 474, No. 667. "The labour lasted thirty hours; the head was firmly fixed in the pelvis, and had made no progress for twelve hours. As the heart's action had some time ceased, and the mother's pulse was 140, the head was lessened, great exertion was necessary to effect delivery, in consequence of the head being much ossified."

(Y) Page 476, No. 740. "Was brought to hospital, reported to have been a considerable time in labour; the pains continued for thirty hours, with little intermission; the labour made but little progress, and the heart having ceased to act for some time, the head was lessened, and delivery completed by the crotchet."

(Z) Page 472, No. 626. "The labour having made no progress for eighteen hours, the head being firmly fixed in the pelvis, and the heart's action having some time ceased, the perforator was used, and delivery completed by the crotchet. It was a first child, the labour lasted forty-three hours."

(AA) Page 477, No. 817. "Was fifty-six hours in labour of her first child, for the last twenty-four of which the head made no progress. The waters were discharged early, the pains were very ineffectual, and the soft parts continued in such a state as to prohibit the use of the forceps. As the child had been now dead some time, as ascertained by the stethoscope, delivery was accomplished by lessening the head.

"She died on the eighth day after delivery, from abdominal inflammation. On dissection, a considerable quantity of fluid, of a yellowish colour, was found in the cavity of the abdomen; the omentum was firmly adherent to the intestines, which were very vascular; there was also an extensive deposition of lymph. The substance of the uterus was very soft, and the ovaries much injured from the effects of inflammation."

(BB) Page 478, No. 820. "Was forty-eight hours in labour, (first child,) for the last thirty-six of which, there was little progress made, and the child having been some hours dead, as indicated by the stethoscope, she was delivered by the crotchet."

(CC) Page 480, No. 976. The labour lasted in this case forty-eight hours (first child); and although uterine action was strong for the last sixteen hours, there was not any progress made. As the foetal heart had ceased to act for some time, the

brain was evacuated, in which state the head was left for six hours, and then brought down with the crotchet.

(DD) Page 480, No. 1032. "Was admitted, in labour of her eleventh child; uterine action very frequent and strong; the os uteri dilated to the size of a crown, and the fundus very much inclined to the right side. In twenty-four hours after admission, (the head not having made any progress for the last eight,) the foetal heart having ceased to act for some time, it was thought advisable to lessen the head, and deliver with the crotchet.

"This was the *fourth* time she had been delivered artificially. Fifteen months since, she was delivered in this hospital with the crotchet."

The only explanation I can offer of this, as it appears to me, most unfortunate practice, is, that Dr. Collins does not seem to have been aware of the importance of ascertaining the position of the infant, and the state of the passages in cases where the labour is at all protracted. He avows his sentiments in the following words: "When uterine action continues regular and strong for twelve, or twenty-four hours after the os uteri is dilated or nearly so, without the child's head making progress; it being firmly compressed in the pelvis, not leaving space for the introduction of the finger to feel the ear, or in some, the passage of a catheter into the bladder; the urine, perhaps, retained from severe pressure on the urethra; or when removed, bloody and very scanty, and of a deep colour; the patient complaining of acute pain on pressure in any part of the abdomen, the pulse being at the same time hurried, and the strength failing; these are symptoms indicating the use of the perforator, and their being *urgent*, or *otherwise*, should make us deliver sooner or later. The difficulty, in such cases, is caused by a disproportion between the child's head and the pelvis; and, except where this is very great, no individual can foretell whether the uterine action may be sufficient or not to expel the child; therefore, the most certain proof we can have of

such disproportion existing is, the head remaining stationary for a number of hours after the dilatation of the mouth of the womb, uterine action during this time continuing strong. This is a more certain proof than any derived from the most accurate examination ; for though in this way we may be able to inform ourselves, with tolerable correctness, as to the size of the pelvis, yet, the size of the child's head, its degree of ossification, or the amount of compression it may undergo from uterine action, never can be correctly ascertained. Let it be carefully recollected, at the same time, that so long as the head advances *ever so slowly*, the patient's pulse continues good, the abdomen free from pain on pressure, and no obstruction to the removal of the urine, interference should not be attempted, unless the *child be dead.*"—*Practical Treatise*, page 16.

To those observations of Dr. Collins, I have to call, very particularly, the attention of young practitioners.

Firstly. My solemn conviction is, that no patient ought to be allowed, where the head of the child comes foremost, to continue so long in labour, that its head should be so firmly compressed in the pelvis, not leaving space for the introduction of the finger to feel the ear, or in some the passage of a catheter into the bladder, &c.

Secondly. Where there is a disproportion either arising from the size of the infant, or its malposition, or from a hand being forced down along with the head, or from narrowness of the pelvis, the fact, according to my experience, can be ascertained by an attentive practitioner, long before any alarming symptoms result from those obstacles. But, in the Dublin Lying-in Hospital, in Case 509, page 470, "the unusual size of the head ;" in Case 470, page 526, "the face having turned under the pubes ;" in Case 49, page 462, "the hand having come down with the head ;" in Cases 126 and 150, pages 158 and 464, the deficiency of space between the ischia ; and in Case, No. 173, page 464, the narrowness at the brim of the pelvis, from the projection of the promontory of the sacrum, had not

been discovered till irreparable injury had been done to the mother and child.

Thirdly. The precept in the preceding quotation, "that so long as the head advances ever so slowly, the patient's pulse continues good, &c.," my experience has led me to reject, and I consider it to have been proved fatally erroneous by the circumstances of the case of the Princess Charlotte. But, as this evidence has been called in question, I shall quote another case, on which, I do not anticipate any objection whatever. (Case thus described by Professor Davis, of the London University, *Elements of Operative Midwifery*, page 148):—"Lady T——, about thirty years of age, of stature rather under the middle size, but, presenting no external deformity of figure, sustained, during her first puerperal confinement, an extremely difficult labour. She was attended by a practitioner in the country, who delivered her with the forceps of a dead child. Her convalescence was very protracted; but, she eventually did recover her general constitutional health. She, however, suffered so much injury of the parts within the pelvis, that she was never able afterwards to retain her urine. After some years, she became pregnant again. At the full period of her gestation the liquor amnii escaped, without any previous indication of approaching labour. This incident was, however, succeeded almost immediately by unequivocal symptoms, viz., the first slight pains of parturition. She was delivered of a still-born child, after a severe labour of eighteen hours' duration. The time here named was the precise measure of its whole duration, from the very first twinge of the struggle to the moment of delivery. During no stage of this labour could it be truly asserted that there was not some progress made. In the course of a few hours from its commencement, the orifice of the uterus gradually dilating, the head of the child began to enter into the cavity of the pelvis; it presented in the first position, and effected its transit through the pelvis, certainly in the midst of such a tempest of struggles, as I think I have never

witnessed upon any other occasion. But, at no period of the process, was there any thing approaching to a suspension of the action of the uterus, or of arrest of the child's head in the passage. There was a constant progressiveness throughout every stage of the labour. Such was the extraordinary excitement of the heart and arteries in this case, that it was thought necessary to prescribe venesection about twelve hours from the commencement of the labour pains. The same operation was proposed to be repeated in about four hours afterwards; but this proposition was, unfortunately, successfully resisted by a young physician, who, for reasons, of which it is not necessary to apprize the reader, possessed a predominant influence in the family. Such, however, was the extreme irritation then present, that in the course of twenty minutes afterwards, the wretched patient became the subject of a severe rigor of upwards of half an hour's duration. The head of the child was at this time bearing strongly on the perineum. In the mean time, however, the throes of parturition were not suspended, nor even perceptibly enfeebled; and, as has been already stated, the delivery was completed within eighteen hours from the commencement of the labour. The patient died on the tenth day after her delivery. On inspecting the body after death, which was obligingly undertaken by my ingenious friend Mr. Henry Gaultier, now Dr. Gaultier, in the presence of Dr. Sims, Dr. Courthope Sims, Dr. —, the gentleman already alluded to, and myself, the cause of the death was discovered to have been a large abscess, which seemed to have implicated all the structures at the superior part of the cavity and towards the left side of the pelvis, and, of which, the left ovarium, probably dangerously contused during the labour, had all the appearance of having been the nucleus."

According to Dr. Collins's records, it appears to me, that there had been, during his mastership, many unfortunate victims to the rule of practice to which I so strongly object.

Dr. Collins, p. 487 of his *Practical Treatise*, gives the

following explanation of the extraordinary proportion of crotchet cases which occurred in the Dublin Lying-in Hospital during his mastership.

“ In this report of the number of children delivered by the crotchet, it is necessary to bear in mind, that the *proportion* of such deliveries is greatly increased in consequence of the *same patient* returning to hospital *two, three, or even more times*, in whom, from deformity or other circumstances, such mode of delivery was rendered *unavoidable*.”

On this practical point, the records of the Edinburgh General Lying-in Hospital, show a very different result, for in cases of deformity premature labour was induced, in the subsequent pregnancies of the individuals, and successfully induced ; and, as I have stated in my Practical Treatise, Part 2, p. 182, that during the limited period in which the records of that hospital have been kept accurately, premature labour had been induced two different times on each of six women with deformed pelvis, and nine infants were born alive.

I have in the same work, p. 180, mentioned, that in private practice, previous to January 26, 1836, I had brought on premature labour forty-five times in twenty-one individuals with deformed pelvis, and that forty-one of the forty-five infants were born alive. When the professional reader compares this result, viz. saving fifty infants out of fifty-seven, in cases where the mother's pelvis was defective, he will admit, that if the facts I have stated be correct, (and I challenge the most scrutinizing inquiry,) I have shown the utmost anxiety both to lessen the sufferings of the patient, and to preserve the life of the infant, instead of cruelly encouraging the destruction of the child, as alleged by Dr. Collins in p. 40 of your Journal for March, 1837.

In concluding these observations, I have great pleasure in declaring my conviction, that the profession and the public at large are highly indebted to Dr. Collins for the publication of his Practical Treatise on Midwifery. It contains a circumstantial and candid detail of certain cases of difficulty and danger,

on the treatment of which, the profession has hitherto been guided by no definite rules. But, I now venture to repeat my solemn opinion, that those cases prove, in the most unequivocal manner possible, the utility and the importance of the precepts laid down in my *Practical Observations*, and that thus, the junior practitioner may be directed in the treatment of protracted labours by rules established, not only by the individual practice of the author, but by the recorded cases of the great Dublin Lying-in Hospital. And if another addition of my work be called for, I shall hold it to be my duty to insert the substance of this article in that work.

I have the honour to be, Sir,

Your obedient, humble Servant,

JAMES HAMILTON.

23, St. Andrew's-square, EDINBURGH,

29th January, 1838.

ART. X.—*Second Medical Report of the Western Lying-in Hospital and Dispensary, 31, Arran-quay.*—By FLEETWOOD CHURCHILL, M. D., Physician Accoucheur to the Hospital, and Lecturer on Midwifery, &c., at the Richmond Hospital School.

[Read before the Surgical Society of Ireland, Saturday, February 10, 1838.]

THE second medical Report of the Western Lying-in Hospital embraces a period of fourteen months, i. e. from the 1st of November, 1836, to the 31st of December, 1837, inclusive.

During that time, 391 females derived assistance from the hospital : of these, 128 were intern ; and 263 extern patients.

From this number we must deduct twenty-one cases of abortion, and one case of uterine hydatids, which will leave 369 cases of labour.

The number of children born amounted to 376, of whom 206 were males, and 170 females ; twenty-eight of them (seven-

teen males, and eleven females) were still-born, or died an hour or two after birth : of these—

- 5 were premature.
- 2 were breech presentations.
- 2 were footling do.
- 2 were funis do.
- 1 was a footling case, with prolapsed funis.
- 1 was covered with syphilitic eruption.
- 1 was a crotchet case.

The ages of 313 women were noted as accurately as possible :—

24	cases	were	under	20	years	of	age.
97	„		between	20	and	25.	
106	„	„		25	and	30.	
42	„	„		30	and	35.	
42	„	„		35	and	40.	
2	„	„		40	and	50.	
Total,				313			

As it is not possible to ascertain the exact commencement of labour in every case, I have preferred omitting altogether from the registry such as were doubtful. On this account I can only give the duration of labour in 299 cases, instead of 369 :—

In 65 cases it was under 6 hours.

93	„	„	12	„
105	„	„	24	„
18	„	„	36	„
7	„	„	48	„
5	„	„	60	„
5	„	„	96	„
1	„	„	120	„

It is but fair to mention, that the responsibility for the

length of the entire labour, as well as of its different stages, in almost every tedious case, rested with the patient or her friends, and not with the medical attendants. No application was made for assistance until they were alarmed by the delay.

The period which elapsed between the setting in of the pains and the rupture of the membranes was noted in 271 cases :—

In 60 cases it was under 2 hours.

58	„	about 6	„
41	„	10	„
47	„	14	„
20	„	18	„
12	„	22	„
14	„	26	„
3	„	30	„
6	„	35	„
1	„	40	„
4	„	50	„
1	„	60	„
3	„	80	„
1	„	108	„

Total, 271

In the same number of cases, the interval between the rupture of the membranes and the birth of the child, was as follows :—

In 136 cases it was under 1 hour.

40	„	about 2	„
46	„	4	„
19	„	6	„
7	„	8	„
3	„	10	„
9	„	15	„
3	„	20	„
5	„	25	„

In 1 case it was about 35 hours.

1	„	„	40	„
1	„	„	50	„

Total, 271

From the birth of the child, until the expulsion of the placenta, there elapsed—

5 minutes in 69 cases.

10	„	54	„
15	„	63	„
20	„	39	„
25	„	3	„
30	„	31	„
35	„	7	„
40	„	9	„
50	„	3	„
60	„	11	„
From 1 to 2 hours,		15	„
2 to 3 hours,		2	„
3 to 4 hours,		2	„
5	„	4	„
6	„	1	„
8	„	1	„

All these cases, where so long an interval elapsed before the exclusion of the after-birth, were under the care of midwives, who applied to the hospital on account of the retention of the placenta.

In 358 cases the presentation was as follows :—

In 331 it was natural.

4 the hand descended with the head.

9 the breech presented.

10 the feet, in three of which the funis prolapsed.

2 the funis presented.

2 the arm.

Total, 358

I have already mentioned that two of the breech presentations, three of the footling, and both of the funis cases, were lost. Of the two arm presentations, one child was lost, and the other saved.

There were seven cases of twins ; their sexes were :—

In 1st case	2 males.	0 females.
2nd „	2 „	0 „
3rd „	1 „	1 „
4th „	2 „	0 „
5th „	1 „	1 „
6th „ (premature)	0 „	2 „
7th „	2 „	0 „
Total, 10		Total, 4

Of these, one male and two females (premature) were still-born, or died immediately after birth.

As to the presentations ; in four cases both the children presented naturally, and one child was still-born. In the fifth case, one child presented the breech, and the other the arm. In a sixth case, one child presented the breech, and the other the head : in each of these cases both children lived. In a seventh case, the presentation of one child was natural, and the hand of the other descended along with its head : both children were lost.

In five cases *hemorrhage* occurred after the birth of the child, before the expulsion of the placenta : in four of these, manual extraction was necessary. The hemorrhage caused no unfavourable results.

One patient was threatened with *convulsions*, but by the prompt employment of antiphlogistic remedies she escaped.

In some few cases, patients were attacked with intestinal irritation, or exhibited symptoms of incipient *hysteritis*, which however subsided under the usual treatment, except in one instance, where a quantity of purulent matter was formed, and escaped '*per vaginam*.'

During the past year, we have had no cases of *puerperal fever* among either the extern or intern patients.

In three cases (one in 123) we were obliged to *turn the child*, in consequence of mal-presentation or prolapse of the funis. All the mothers recovered, and one child was saved.

The *forceps* (one in 369) were applied once successfully : both mother and child recovered.

In one case the *perforator* was used (one in 369), on account of pelvic narrowness : the woman recovered.

Of the 391 females attended in the hospital or at their own homes, three only died (or one in 130), and two, at least, of these, owed their deaths to circumstances which could never have occurred had they been delivered in the hospital. The third sunk from the shock of a tedious labour upon a constitution enfeebled by severe pulmonary disease.

Having terminated the numerical details taken from the hospital registry, I shall now proceed to enter upon some more general considerations, and to relate shortly some of the most interesting cases.

During half of the past year, two very intelligent pupils of mine (Mr. John Gray, and Mr. Charles Gibbon) kept accurate registries of several points in the history of natural labour, about which I was anxious to obtain numerical information.

We endeavoured, by taking down a considerable number of cases, to ascertain the effects of the nervous shock (caused by labour) upon females of different temperaments, after labours of various duration, and under ordinary and extraordinary circumstances. The pulse was examined just before delivery, at the completion of parturition, and at short intervals afterwards, and its variations and alterations noted.

The changes in the quantity, colour, and consistence of the lochial discharge were investigated.

The gradual contraction and recession of the uterus was appreciated, and the period of the secretion of milk carefully entered in the registers.

Although the number of cases, thus minutely investigated, was not so great as to preclude further inquiry, yet much information was obtained. By comparing these observations, we

were enabled not merely to ascertain with considerable accuracy, the progress of convalescence after natural labour with the successive phenomena, but were also made acquainted with certain deviations from the usual course, which were of importance, although involving no organic disease.

The results of these investigations were published in the *Dublin Journal* for September, 1837.

TEDIOUS LABOUR.

In the Report of the Hospital for 1835-6, I took occasion to allude to the question of how far the length of the first stage of labour may influence the duration of the second stage, and the consequences to the mother and child. Taking the rupture of the membranes as the limit of the first stage, (which is generally, though not always, the case,) and referring to the tabular views I gave of the intervals from the commencement of labour to the rupture of the membranes, and from the rupture of the membranes to the birth of the child; I drew the following conclusions:

“1st. That the length of the period after the evacuation of the liquor amnii, bore no proportion to the time which elapsed previously. 2nd. That the constitutional effects of labour are not to be estimated by the length of the first stage, &c.”

Fully believing in the accuracy of these opinions, it nevertheless struck me, that it might be more satisfactory if some further analysis were undertaken. I have accordingly taken, indiscriminately, twenty-one cases of labours of thirty-six hours' duration and upwards, and have marked down the duration of each stage, and the issue of the labour to mother and child.

Nine Cases of Labour of Thirty-six Hours' Duration.

No. of Cases.	First Stage.	Second Stage.	Results to	
			Mother.	Child.
In 5 cases.	35 hours.	1 hour.	Favourable	Favourable.
2 do.	34 do.	2 do.	Do.	Do.
1 premature.	32 do.	4 do.	Do.	Still-born.
1 case.	25 do.	11 do.	Do.	Favourable.

Four Cases of Labour of Forty-eight Hours' Duration.

No. of Cases.	First Stage.	Second Stage.	Results to	
			Mother.	Child.
In 1 case.	47 hours.	1 hour.	Favourable	Still-born funis presentation.
1 do.	47 do.	1 do.	Do.	Favourable.
2 do.	45 do.	3 do.	Do.	Do.

Six Cases of Labour of Sixty Hours' Duration.

No. of Cases.	First Stage.	Second Stage.	Results to	
			Mother.	Child.
In 3 cases.	59 hours.	1 hour.	Favourable	Favourable.
1 do.	57 do.	3 do.	Do.	Do.
1 do.	53 do.	7 do.	Do.	Do.
1 do.	39 do.	21 do.	Do.	Do.

Three Cases of Labour of Ninety-six Hours' Duration.

No. of Cases.	First Stage.	Second Stage.	Results to	
			Mother.	Child.
In 1 case.	95 hours.	1 hour.	Favourable	Favourable.
1 do.	93 do.	3 do.	Do.	Do.
1 do.	90 do.	6 do.	Do.	Do.

This table appears quite conclusive as to my first proposition; for out of twenty-one cases of labour, varying in duration from thirty-six to ninety-six hours, in only four, did the second stage amount to more than four hours, while in eleven it was concluded in one hour. Neither did the duration of the second stage increase in proportion to the prolongation of the whole labour, for of the three cases of ninety-six hours each, in only one, did the second stage exceed three hours.

These cases also afford at least negative evidence in support of the *second* proposition, for although in all but one, it was the prolongation of the first stage which caused the delay, and

although in some this first stage was both excessively long and badly managed, (from being intrusted to ignorant females,) yet in no instance did the constitutional symptoms which characterize powerless labour arise; and this I fully believe, because it was during the first, and not the second stage, that the delay took place.

Further, it will be perceived at once, that these tables have a peculiar bearing upon the interesting controversy between Dr. Hamilton of Edinburgh, and Dr. Collins of Dublin. The opinions of each of these eminent practitioners are entitled to great respect, from the extensive opportunities they have enjoyed, and from the profound sagacity they have brought to bear upon the question.

So far as the series of facts I have just presented, extend they are in direct opposition to the recorded opinions of Professor Hamilton; for a prolonged first stage neither rendered “the powers of the uterus inadequate to expel the infant with safety to its life, or to the future well-being of the patient;” “nor disposed the uterus to contract irregularly, so as to occasion retention of the placenta;” “nor too feebly to prevent fatal hemorrhage;” “nor lastly, did it give rise to febrile or inflammatory affections of a most dangerous nature.”*

For 1st, all the children were expelled alive and continued to live, except two, one of which was premature (six months), and the other presented with the funis, and whose deaths were consequently not attributable to the protraction of labour. 2ndly, neither flooding, retention of placenta, fever, nor inflammation happened in any case; on the contrary, every one of the cases recovered as well as after an ordinary labour of twelve hours’ duration.

The causes of delay in these cases were generally such as are enumerated in midwifery works, and with especial clearness by Dr. Hamilton:—premature evacuation of the waters, rigi-

* See Hamilton’s Practical Observations, Part I.

dity of the soft parts, depression of the anterior lip of the os uteri, &c. &c. &c., and the treatment usually recommended was employed successfully.

One of the fatal cases was an instance of tedious labour, but the result was as much to be attributed to the pulmonary disease under which she had suffered some time, as to the character of the labour; she was admitted into the hospital on the evening of the 8th April, 1837. She had some pains, which completed the first stage of labour in twenty-six hours, without the slightest exhaustion, or acceleration of pulse. The progress of the head of the child, after entering the upper strait, was slow though quite perceptible, during the next twelve hours, but as the second stage was so prolonged, it was deemed advisable to call in Dr. Darley, the consulting accoucheur, who gave it as his opinion, that the patient's strength was adequate to her delivery, and that her condition did not call for interference. Two hours after this time, a male child was born, and the placenta was expelled half an hour subsequently. The pulse, immediately after delivery, was about 120, and she seemed much exhausted by the length of the labour, and by the distressing cough. She sank on the third day, without any very prominent symptoms.

VERSION CASES.

It was found necessary to turn and extract the child by the feet in three cases. In the first case, an arm presented, with a loop of the umbilical cord. The waters had been discharged an hour and a half before assistance was obtained, and the uterus was acting briskly. Mr. Speedy succeeded, after some time, in extracting the child, which was still born. The woman recovered well.

The second case was one of twins, the first child presented with the breech, and was born alive, after a short labour. On making an examination, Mr. Speedy found a second child presenting an arm; he turned it instantly, and succeeded in extracting it *alive*.

The operation was facilitated, of course, by the transmission of the first child. Both children, with their mother, did well.

The third case was a case of funis presentation, admitted into hospital June 12th, 1837. The waters were discharged about three, P. M., and then the prolapsed cord was discovered. On my arrival, at four, P. M., I found the funis still pulsating, and as the pelvis appeared sufficiently large, delivery by the feet seemed to afford the child a chance for its life. I succeeded in turning and bringing down the lower extremities and body very readily, but so much time elapsed before the head was disengaged, that the infant was lost. The woman recovered well.

FORCEPS CASE.

But one forceps case occurred in 391 cases; it is entered in the case book as follows: M. Broderick, æt. twenty-eight, was taken in labour of her first child, at two, P. M., June 28th, 1837. The midwife in attendance sent for the assistant surgeon of the hospital, Mr. Speedy, to pass the catheter at five, P. M. June 29th. Upon inquiry, he found that the waters were discharged at three, A. M., of that day, and that the head, shortly afterwards, descended into the cavity of the pelvis. A pint of high-coloured urine was drawn off, and a purgative enema administered. At nine o'clock in the evening, the head had made no advance, having remained in the same situation for twelve hours; the pains had diminished in force and frequency, the patient was a good deal exhausted, the pulse 120 and weak, skin hot and dry, tongue white, with great thirst. A consultation was immediately held, and instant delivery resolved upon. As there appeared sufficient space in the pelvis, it was determined to apply the forceps, and in a short time Mr. Speedy extracted a *living child* without injury. The placenta was expelled in twenty minutes. Both mother and child recovered well.

CROTCHET CASE.

The perforator was used once, under very unfavorable circumstances. The patient, Mary Corr, æt. 30, had suffered from ill health during the latter months of her pregnancy. We were informed that she was taken in labour on the 7th of April, 1837, and that the pains were slight for two days, but increasing, when they were entirely suspended, in consequence of a quarrel with her husband. They returned some time the next day ; and on the 12th Mr. Speedy visited her : he found the head presenting and impacted firmly in the upper strait ; the liquor amnii had not been discharged ; the bowels were constipated, and the urine retained ; the pulse was quick and the skin hot and dry. The catheter was passed, and a quart of foetid urine drawn off : a purgative enema was given, and the membranes ruptured. The uterine action increased, and strong pains recurred every five minutes. A consultation was held in the evening, and as all the constitutional symptoms had increased very much, without any advance of the head, it was deemed hazardous to delay the delivery any longer. The head was lessened and the child extracted, followed in a short time by the placenta. The woman recovered without a bad symptom. The history of this case is as indistinct as the early management was faulty, and owing to the same cause, viz., the stupidity of the female attendants, and the drunkenness of her husband.

Of the cases of *Retained Placenta*, there were two, of which a slight notice may be given. The first occurred after a premature labour at the sixth month. The patient sent to the hospital twelve hours after the foetus was expelled, in consequence of flooding having come on. It was found impossible to extract the placenta, and the plug was therefore used, with success as far as the hemorrhage was concerned. The plug was changed daily, and purgative enemata given, but without detaching the secundines until sixty hours after the expulsion of the foetus. During the latter part of this time, the patient became feverish, with

quick pulse, headach, hot skin, &c. ; but these symptoms were dissipated by small doses of calomel and Dover's powder, with gentle purgatives. Vaginal injections of warm water were given daily, and the patient shortly recovered.

The second case had a less favourable issue, though that was from causes unconnected with parturition. She was delivered of twins in the seventh month of pregnancy, under the care of a midwife. The first child was born after a labour of twenty-four hours' duration, at nine o'clock, A. M., August 10, 1837, and the second at noon of the same day. The first presented the head, and the second the hand and head. Between the birth of the children, and subsequently before the expulsion of the placenta, very considerable hemorrhage occurred ; but some time elapsed before the midwife sent for assistance. Mr. Speedy found the patient greatly exhausted from loss of blood, the surface pale and cold, and the pulse almost imperceptible. Having administered a cordial, he extracted the placentæ, and the patient rallied comfortably. Two days afterwards she was attacked with abdominal irritation, which was removed by venesection, calomel and opium, &c., so that when visited on the morning of the 17th instant, she was reported convalescent. That same evening she got out of bed and quarrelled with some of her lodgers, after which she drank some punch and lay down. She was attacked in the night by rigors, abdominal pain, &c. &c. and in the morning, presented symptoms of peritoneal or uterine inflammation, which resisted the usual remedies, and she died during the night of the 18th. We obtained permission to examine the abdomen ; but nothing was discovered which would account for her death. The viscera were all healthy ; there was not a trace of inflammation in the uterus or peritoneum.

With regard to the third fatal case, I may add a few words. She was delivered safely of an *acephalous* foetus, after a natural labour, and was progressing satisfactorily, until some imprudent person showed the child to her : the shock was very great ; she

was taken ill immediately, and notwithstanding the most active treatment, she died with symptoms of intestinal inflammation. We could not obtain a post mortem examination.

The particulars of the case of *Uterine Hydatids* are as follows:—The patient, Ann Curwen, æt. 27, the mother of two children, generally enjoying good health, menstruated regularly up to the end of August, 1836; the menses ceasing from this time, from pregnancy, as she believed. About a month afterwards, however, she observed a slight discharge from the vagina resembling blood and water, which continued three months or more, up to December 18, 1836, when she was attacked with labour pains and all the signs of abortion, except that instead of an ovum, a large basin full of hydatids was expelled, with considerable hemorrhage. The patient recovered under the ordinary treatment.

The measurements of the cord have been continued; but as the results do not differ from those detailed in a paper published in the *Dublin Journal* for March, 1837, I do not deem it necessary to repeat them here.

If the statistics given in the last Report be added to those of the present year, the result is not without interest.

Number of females delivered, 638: intern, 215; extern, 413; abortion, 33; leaving 605 cases of labour.

616 children born: 340 males, 276 females; 11 cases of twins; 49 still-born, or died soon after birth—31 males, 18 females. Of these,

- 6 were premature.
- 4 were breech presentations.
- 5 were footling.
- 2 were funis.
- 3 were arm.
- 4 were crotchet cases.
- 1 footling case, with prolapsed funis.
- 1 syphilitic.

Ages of 534 Women.

47	under	20	years.	
168	between	20	and 25	years.
188	„	25	„ 30	„
67	„	30	„ 35	„
59	„	35	„ 40	„
5	„	40	„ 50	„

Duration of Labour in 513 Cases.

Under	6	hours	in	133.
About	12	„	„	164.
„	24	„	„	151.
„	36	„	„	33.
„	48	„	„	13.
„	60	„	„	8.
„	96	„	„	10.
„	120	„	„	1.

Interval between Commencement of Labour and Rupture of Membranes in 473 Cases.

Under	2	hours	in	104	cases.
About	6	„	„	189	„
„	10	„	„	85	„
„	14	„	„	74	„
„	18	„	„	31	„
„	22	„	„	14	„
„	26	„	„	18	„
„	30	„	„	4	„
„	35	„	„	13	„
„	40	„	„	6	„
„	50	„	„	6	„
„	60	„	„	5	„
„	80	„	„	3	„
„	108	„	„	1	„

*Interval between Rupture of Membranes and Birth of Child, in
473 Cases.*

Under 1 hour in 259 cases.

About 2 hours in 67 „

„ 4 „ „ 61 „

„ 6 „ „ 28 „

„ 8 „ „ 13 „

„ 10 „ „ 10 „

„ 15 „ „ 16 „

„ 20 „ „ 4 „

„ 25 „ „ 9 „

„ 30 „ „ 2 „

„ 35 „ „ 3 „

„ 40 „ „ 1 „

„ 50 „ „ 1 „

Interval between Birth of Child and Expulsion of Placenta.

5 minutes in 145 cases.

10 „ 106 „

15 „ 102 „

20 „ 54 „

25 „ 4 „

30 „ 38 „

35 „ 8 „

40 „ 10 „

45 „ 2 „

50 „ 3 „

60 „ 14 „

From 1 to 2 hours, 18 „

„ 2 to 3 hours, 5 „

„ 3 to 4 hours, 2 „

5 hours, 4 „

6 „ 1 „

7 „ 1 „

Presentation in 582 Cases.

In 545 it was natural.

5 the hand descended with the head.

14 the foot presented : 5 were lost.

11 the breech „ 4 „

4 the arm „ 3 „

2 the funis „ 2 „

1 the placenta.

6 cases of turning ; 1 in 100 ; 1 child, and all the mothers saved.

1 forceps case ; 1 in 605 ; mother and child recovered.

4 crotchet cases ; 1 in 151 ; all the mothers recovered.

7 cases of hæmorrhage.

4 women died, or 1 in 151.

I cannot conclude without expressing my sense of the obligations I am under to the consulting officers of the hospital, and to my colleagues, for their willing and efficient co-operation : to their assistance must be chiefly attributed the favourable results which have attended our exertions.

ART. XI.—*Report of the Newry Dispensary and Fever Hospital for the Year 1837.* By J. MORRISON, M. D., M. R. C. S. L., Physician to the Newry Dispensary and Fever Hospital, &c.

THIS institution consists of a dispensary and hospital : the former affords advice, medicine, and, when necessary, attendance to the sick poor of the town and neighbourhood ; and the latter is for the reception of fever and bad accidental cases—the finances restricting the admissions, unless in very urgent cases, to these two classes of disease.

As the hospital and dispensary are in every respect intimately united, the patients of the one very frequently becoming

those of the other, and the out-cases of importance being attended to equally with the intern, I have thought it best, for the sake of plainness, in making out the following cursory sketch, to speak, with a few exceptions, of the two departments, as if they were completely one.

The following is a statement of the diseases which were treated in both during the year.

	No.	Dead.		No.	Dead.
Abortion,	5		<i>Brought forward,</i>	277	12
Abscess	14	1	Dysentery,	5	1
Amaurosis	2		Elephantiasis,	1	
Amenorrhea	7		Epilepsy,	2	
Aneurism, by anastomosis and temporal,	4		Erysipelas,	8	
Anus, prolapsus and fistula of,	3		Eversion of eyelids,	2	
Apoplexy,	2	1	Eye, inflammation and other affections of,	20	
Asthma,	3		Fever, intermittent,	2	
Bladder, irritable and in- flamed,	4		—, puerperal,	4	
Blood, vomiting of,	3		—, remittent, of infants,	2	
—, spitting of,	4		—, typhoid,	507	10
Brain, concussion of,	3		Fistula, lachrymal,	3	
—, inflammation of,	2	1	Fractures,	32	
Bronchitis, acute and chro- nic,	21	2	Glands, enlarged,	5	
Burns,	7	1	Gleet,	5	
Cancer,	7	1	Gonorrhœa,	9	
Carbuncle,	1		Hare-lip,	2	
Cataract,	3		Heart, organic and nervous affections of,	7	1
Chlorosis,	13		Head-ach, inveterate,	2	
Cholera, common,	7		Hemicrania,	2	
Cynanche,	11		Hernia, strangulated,	1	
Contusions,	15	1	Hooping cough,	7	
Convulsions, infantile,	5		Hydrocephalus, acute,	5	1
—, puerperal,	2		—, chronic,	2	
Cutaneous diseases,	35		Hysteria,	5	
Croup,	3	1	Influenza,	176	9
Crowing disease,	2		Inflammation, phlegmonoid, — of bowels,	5	1
Dentition, diseases of,	4		— of stomach,	3	1
Deafness,	2		— of pleura,	9	
Diabetes,	1	1	— of liver, acute,	2	
Dyspepsia,	25		—, chronic,	4	
Diarrhœa,	27		— of lungs,	9	2
Dislocations,	17		— of kidney,	1	
Dropsy of abdomen, chest, testicle, and cellular,	13	2	— of peritoneum,	5	
	277	12	— of periosteum,	6	
				1141	38

	No.	Dead.		No.	Dead.
<i>Brought forward,</i> .	1141	38	<i>Brought forward,</i> .	1309	45
Joints, inflammation and other affections of, .	9		Scarlatina,	9	
Labour,	45		Scalds,	16	1
Leucorrhœa,	5		Scirrhus of pylorus, . .	1	
Marasmus,	6		—— of mamma,	2	1
Measles,	17		Scrofula,	27	
Menorrhagia,	7		Small-pox,	5	1
Neuralgia of face, thorax, &c.,	11		Spine, affections of, . .	3	
Necrosis,	3		Sprain, severe,	4	
Palsy,	5	1	Syphilis,	13	
Perinæum, lacerated, . .	4		Testicle, indurated, . .	2	
Paraphymosis,	3		Tonsils, enlarged, . . .	3	
Phthisis,	9	6	Tumours,	10	
Phlegmasia dolens, . . .	2		Ulcers,	20	
Phlebitis,	1		Urine, retention of, . .	1	
Piles,	8		Vomiting, obstinate, . .	1	
Polypus, nasal,	2		Whitlow,	3	
Pregnancy, diseases of, .	5		Worms,	31	
Puerperal diseases, . . .	3		Wounds, lacerated and punctured,	13	
Rheumatism,	23		——, gun-shot,	1	
	1309	45	Total,	1474	48

During the first two or three months of the year, *influenza* prevailed in our town to a great extent. The poor, from a deficient supply of clothing, fuel, and nutriment, together with the severe season, suffered extremely from its effects. The debility induced by these causes was very great, and rendered recovery not only invariably slow, but frequently very uncertain. Those in the prime of life, who were the most frequent subjects of its attack, usually recovered from the disease; but the mortality amongst the aged and infirm, both of the dispensary patients and those throughout the town generally, if the ordinarily supposed innocuousness of the affection be taken into account, was very considerable. Some seemed to sink from sheer nervous depression, but the majority, with excessive secretion of the bronchial mucous membrane, from asphyxia.

Whether the situation of our town, which is nearly surrounded by hills, and the greater part low and damp, had any

particular effect in inducing and keeping up excessive debility, with bronchial irritation, I cannot decide ; but these were here constant and long-continued sequels of the disease.

With regard to the treatment, it was generally found that, with the exception of aperients and diaphoretics, the usual anti-phlogistic measures, even at the commencement of the disease, could not for any length of time be safely employed, their effects being soon recognizable as very different from those which accrue from their administration in pure inflammatory affections of the respiratory organs.

After the first two or three days of the incursion, there was seldom any risk in using tonics and stimulants ; and this treatment I found to be by far the most successful. Under the tonics, I would comprise nutritive broths, wine, and, in some cases, the preparations of iron, of bark, &c. And under stimulants, the decoction of polygala seneka, in conjunction with carbonate of ammonia, and camphorated tincture of opium, as well as blisters, mustard cataplasms, rubefacient liniments, &c.

In some cases, when great debility existed, with excessive secretion from the bronchi, acetate of lead seemed to form a highly valuable adjuvant to the list of internal remedies. Several persons, labouring under "suffocative catarrh," certainly appeared to be rescued from the jaws of death by the administration of an emetic of mustard, and this followed up by a combination of acetate of lead and camphorated tincture of opium, together with external stimulation over the chest.

Five hundred and seven of the fourteen hundred and seventy-five persons who were treated at this institution, during the time comprised in this Report, laboured under *continued fever*. This disease prevailed extensively throughout the town and neighbourhood during the year, but more particularly during the months of March, April, May, June, and July. Indeed, influenza seemed to retire as if but to give place to that disease. It was almost exclusively confined to the poorer classes,

and its type was evidently typho-epidemic. Its characteristics were :—

1st. A lengthened duration of the first stage, or that of nervous depression : this generally lasted from five to eight days.

2nd. A moderate degree of excitement, and this of short continuation, composing the second stage.

3rd. The almost invariable accompaniment of livid petechiæ, (unless in very young subjects), which usually appeared about the tenth or twelfth day.

4th. Cerebral obtuseness rather than delirium ; the latter, however, occasionally occurred with great severity.

5th. The prolongation of the disease to at least the twentieth day. The pulse was generally small and weak, and ranged from 115 to 130 ; the tongue was mostly dry and brown.

Though the gastro-intestinal mucous membrane was frequently the seat of irritation or inflammation, it seemed by no means constantly so, the symptoms denoting these affections being often confined to the head or chest. But in many instances no uneasiness, at least none which could lead to the supposition of any inflammatory symptoms being present, could be detected in any of the three great cavities.

The remote causes of epidemic typhoid fever I have always thought to exist in the atmosphere, and to be derived from decomposing organic remains. And may not these emanations, from their being subject to a vast variety of modifications, in a great measure give rise to the character of the fever, with respect both to its severity and to the chief seat of its action.

As to the contagiousness of the disorder, I have not the slightest doubt. When a member of a family was removed to hospital at the onset of the disease, and previous to any others of the family being attacked, the latter were almost uniformly exempt from the affection ; whereas, if the person first so seized were allowed to remain in his own dwelling, the disease uniformly extended itself. Of this, abundant proof was almost daily afforded.

Autopsy could be had recourse to only in three cases ; and it is rather remarkable, that in each of these a different locality was found to be the seat of organic disease : cerebral turgescence, with effusion, being almost the only morbid symptoms in one ; hepatization of the lung, with deposition of lymph and a vascular state of the lining membrane of the bronchi, those of another ; and of the third, an intensely injected state of the mucous membrane of the ileum and cæcum.

As to the treatment, no exclusive mode was adopted. Regarding fever as a disease of the whole system, and knowing that organic lesion, which is now-a-days so much spoken of, as being supposed to constitute its essence, has here no definite locality, the head, the chest, and the abdomen, and even different parts and textures in each of these cavities being occasionally the seat of this supposed proximate cause, and knowing that lesions similar to those which arise in fever are frequently found to exist, and even come to a fatal termination, when no symptoms could be detected ; and also contrasting the enfeebled state of the nervous and sensorial functions, with which continued fever invariably commences, with the local pain and absence of nervous depression which obtain in the incipient stages of inflammatory diseases, such as enteritis, pneumonia, &c. Weighing all these circumstances, I cannot but regard continued fever as a disease of the entire system, and the local lesion which is occasionally met with, as a mere accident. Indeed, if it be considered that vitiated secretions are in constant contact, during the course of a low fever, with a delicate surface, such as is mucous membrane, and the vitality of which, from the then weakened state of the body, is likely to be very low, I think it need only excite surprise that structural changes are not almost universally met with in this disease.

From these considerations, and believing that typhoid fever, when once established, has a course to run, no antidote being hitherto discovered to arrest its progress, I have always thought the disease should be treated, not for its name, but for its

symptoms, and that success in the management of this affection, as well as every other, will chiefly depend on our narrowly looking out for symptoms, and combating them as they appear. Neither, I think, should exclusive attention be paid to any part, the head, chest, abdomen, &c., claiming an equal share.

The treatment which was adopted in the fever to which I now allude, and which certainly seemed very successful, (though I, of course, ascribe the low mortality, one in fifty, in a chief degree, to the mildness of the epidemic), principally consisted in endeavouring to promote an equilibrium between excessive and defective action, and to induce a healthy state of whatever organs had been deranged. When general excitement prevailed, without local pain, cold or tepid ablutions, mercury, and antimonials were had recourse to. When local pain existed, bleeding, or blistering, or both, according to circumstances, were adopted in all parts so affected. In a few cases, where high cerebral excitement, with injection of the conjunctiva, &c. prevailed, I found the application of twelve or fifteen leeches to the temples, followed immediately at the cessation of bleeding by a stream of cold water directed over the shaved scalp, and a few nauseating doses of emetic tartar, to have more immediate and decided good effects, than any other treatment which I had previously tried. In the cases of irritable, sleepless delirium, I cannot speak in sufficiently eulogising terms of the benefit which accrued from the use of emetic tartar and opium. Most certainly, by the introduction of the combination of these medicines into the treatment of certain stages of fever, Dr. Graves has conferred a vast benefit on the community.

When diarrhœa existed without much local uneasiness, *hydrargyrum c. creta* and *pulvis ipecacuanhæ*, given in small and repeated doses, together with the application to the abdomen of one or two mustard cataplasms, seldom failed to procure relief. If the irritability was considerable, the compound powder of *ipecacuanha* was substituted for the simple. I may here mention the utility which I so often experienced from mustard

poultices. The celerity of their action, the frequency *with* which, and the extensive surface *to* which they can be applied, and their not affecting the bladder, render them highly serviceable in the treatment of a variety of intestinal irritations, particularly when the dark brown or black tongue, and other symptoms of great prostration, might involve a hazard in even topically detracting a small quantity of blood.

Knowing that "mercury has a peculiar influence in arresting inflammation, and removing and repairing its effects," this medicine was made the basis of those used in the general treatment. Its milder preparations, however, were mostly resorted to, and they were seldom given to such an extent as to affect the mouth. A powder containing five grains of hydrargyrum c. creta, the same of pulvis rhei, and one grain of pulvis ipecacuanhæ, given night and morning, was in ordinary cases found to answer almost every purpose. If the bowels were not sufficiently acted on, a little castor oil was occasionally administered; but generally, the combination of the mercury, rhubarb, and ipecacuanha in the above proportions, acted on the bowels once or twice daily, and at the same time seemed to promote and improve, in an especial manner, the secretions both on the internal and the external surface of the body.

General bloodletting was seldom, if ever, had recourse to. It is a remedy which I would very much dislike to make trial of to any extent in *Irish epidemic* fever, especially in the persons of the poor, the enervated, and the debauched; and those are they who usually suffer from this disease. It is remarkable, that four of the ten who died of fever, had been blooded previous to their becoming dispensary or hospital patients; and in no other case of the 507, was phlebotomy employed. Venesection is much too popular a remedy, at least in this part of the country; indeed so much so, that it is almost a matter of course for the poorer classes to apply for its being adopted at the onset, or during the course of nearly every indisposition.

In several cases, the depression of all the vital powers

seemed so great throughout the entire course of the disease, as to require treatment almost exclusively stimulant.

Before I close these few unconnected observations on fever, I wish to advert to a symptom which was manifested in some five or six cases, and which I am not aware of as having been previously observed in this country—I mean priapism. My attention was directed to it, in the first two cases, by accident; but since these, I have inquired for it, and found it to exist, under certain circumstances, rather frequently. In two of the cases in which it existed, there was considerable heat at the lower and back part of the head. Leeching to the occiput, followed by cold lotions and blistering, together with calomel and emetic tartar, and morphine draughts at bed-time, in these removed the affection. In the three or four other cases, no unusual heat at the back of the head was felt; and in these, blistering, succeeded by tincture of opium and emetic tartar, also completely removed the priapism, though one case terminated fatally; and unfortunately no examination would be permitted.

If priapism in fever, even though its occurrence be rare, be an indication of an affection of the cerebellum, in obscure cases it may be worth looking after, and I mention it now merely for the purpose of eliciting a farther inquiry as to its cause and the frequency of its occurrence in this disease.

The only cases of *dysentery* which were met with, five in number, all occurred in the same family. This family, consisting of a man, his wife, and four children, lived in a cellar which only comprised one apartment. The woman was first taken ill; about a fortnight afterwards, the eldest son became affected; a few days after this, the husband; and soon after him, two others of the children. The youngest child, aged about fifteen months, escaped. From the want of a convenient place for emptying the slops, a large metal pot or boiler was used as a close-stool by all the family, with the exception of the youngest child. A week was usually required for filling this vessel, and

during the illness of the family it was never emptied unless when full.

In each of these five cases the same symptoms were presented, viz., mild low fever, severe griping and tenesmus, with bloody dejections. There were no other persons suffering from dysentery in the same neighbourhood, nor, I believe, throughout the town. Now, though I have not been a believer in the contagiousness of this affection, my opinion on this head has been considerably shaken by the circumstances attending the above cases. It is seen that five out of six of this family became affected with a similar disease, after short successive periods, and without any apparent unusual cause, as regards food, clothing, locality, &c., and without having been exposed to that disease in any quarter, except that of their own residence. And it is also seen that these five persons inhabited only one apartment, and made use of the same chair. Does not this look like contagion? And if so, whether are we to ascribe the youngest child's immunity from the disease, to his extreme youth acting as a prophylactic in a general way, or to his not having come into actual contact with the dysenteric matter?

I think I have found this disease best treated by the occasional exhibition of gentle aperients, such as castor oil or rhubarb, the constant use every four, six, or eight hours, of pills containing in each dose half a grain of opium, two grains of blue pill, and four or five grains of ipecacuanha, (the administration of this quantity of the latter medicine, if the stomach will bear it, and it generally will, I consider in several points of view of great importance,) and the daily application of mustard poultices round the course of the colon.

For the relief of affections of the stomach, which are usually denominated *dyspepsia*, there were constant applications. The few irregular and innutritive meals on which the poor subsist, and which frequently consist of potatoes and salt herrings, or potatoes and common salt, washed down with cold water, and these sometimes attainable only twice during the twenty-four

hours, seem quite sufficient to so derange the stomach, as to induce a host of maladies. Pain, acidity, flatulence, loss of appetite, distention, and the vomiting of sour water an hour or two after food, are the usually complained of symptoms, as also the sensation of pain when pressure is made on the epigastrium. Whether those are entitled to the appellation of dyspepsia, or chronic gastritis, I shall not decide ; but this I know, that they were treated on the supposition of their belonging to the latter affection, with the best results.

The same causes which seem to give rise to dyspeptic affections amongst the poor, appear also to induce several others, of what may be called the staple diseases of our dispensary. Similar causes seem also to lay the foundation for the participation of a variety of diseases in a scrofulous character ; tumours, ulcers, and a host of other affections, being constantly met with, which, though in the abstract they cannot receive the denomination of scrofula, resemble that disease so closely in their nature, as to leave but little doubt of their almost identity with it. Real scrofula, however, is too common an affection, and I have little doubt that from the above causes it is not only frequently acquired, but is also one material source from which a great portion of the human family became deteriorated.

There were two cases of *laryngismus stridulus* ; they both occurred in children, one aged ten months, the other a year, of lax and apparently strumous habits. In one, the abdomen was tumid, with evident enlargement of the mesenteric glands, but without any enlargement of those of the neck ; the ordinary symptoms of gastro-intestinal irritation were very manifest, and the fits were attended with the carpo-pedal spasm. In the other, no enlargement of any of the glands, either of the neck or mesentery, could be detected ; but from the appearance of the tongue and fæces, it was evident there existed considerable derangement of the digestive functions. No other obvious symptoms were in either case evident. From the unsettled state of the pathology of this disease, my attention, in employing

remedial measures, was chiefly directed to the parts whose functions were plainly disordered. In the first case, or that in which enlarged mesenteric glands and gastro-intestinal irritation were so evident, hydrargyrum c. creta, with minute doses of Dover's powder, were administered, and leeches, fomentations, and emollient poultices were applied to the abdomen.

In the other case, powders composed of hydrargyrum c. creta, rhubarb, and small doses of ipecacuanha, were given during the day, and at night a warm bath, and a couple of drops of the solution of acetate of morphia. In the former case, the croupy respiration had completely subsided in a week, and did not return. In the latter, the fits were obviously diminished, with regard both to their frequency and their severity, after two or three days' use of the means employed, and were entirely removed in the course of a fortnight. I should mention, however, that in both cases the gums, wherever unusually red or tumid, were freely incised.

From gastro-intestinal derangement presenting itself in the above cases so very unequivocally, from the croupy breathing subsiding, I may say *pari passu*, with the subsidence of that derangement, from the digestive tube of children presenting such a "focus of irritability," and from the sympathy which so obviously exists between the larynx and the stomach and intestines in many of their disordered states, I am strongly induced to look to the abdomen, even in opposition to the opinions of some eminent writers on this disease, as the source from which this malady springs.

A case of *elephantiasis* of the leg, which occurred in a young man, 23 years of age, was admitted into hospital. He stated that he had been troubled with this disease during the last seven years, that it came on slowly and imperceptibly, and without any feeling of heat or inflammation, that he never had fever, nor any disorder to which he could attribute the affection, but that he was frequently in the habit of sleeping in the open air, his occupation being that of a wandering mendicant, and

that he never had worn shoes or stockings. The leg was nearly three times larger in diameter than the other, and the foot was increased in a proportionate degree. The integuments were of a dark colour, very thick and indurated, and seemed to be formed in one place of scabby, in another of warty, and in another of bristly incrustation, but were perfectly free from all inflammatory symptoms. The inguinal glands were not enlarged, and the general health was very good.

From the disease in this case assuming so much of a local character, from its arising without the least trace of inflammatory action, and without any indication of its being connected with a diseased state of the lymphatic vessels or glands, or of any other part of the system, and from the general state of the constitution being good, I thought it possible, if any means could be devised to effect the removal of the morbid integuments, without materially injuring the subjacent textures, that a feasible ground might be entered on, as regarded the chance of a cure. With this view I had the leg plunged into a bucket of boiling water, and intended to have it kept therein, as long as the heat could be well borne, but unfortunately for the due trial of the experiment, the patient, from mere terror, and not from pain, as he afterwards acknowledged, withdrew his leg almost immediately, or before the heat had time to penetrate the incrustated integuments, and he could not again be prevailed on to immerse it.

Being most anxious, by some means or other, to promote the separation of the diseased tunics, I had the leg now well wetted over with strong nitric acid. The pain endured by this application was by no means so severe as I expected; however, it was such as to admit of an excuse for telling the patient that some water would be poured over the limb, (for the supposed purpose of relieving it). Two or three jugs full of boiling water were therefore quickly effused over the parts to which the nitric acid had been applied, and the pain was now so great as to promise the accomplishment of the desired effect. In a

few days, accordingly, a considerable portion of the least dense of the integuments were thrown off, but the thicker or more warty remained. The patient was again solicited to immerse his foot in boiling water, but he obstinately refused, and forthwith decamped from the hospital.

I shall here merely observe, that I exceedingly regret my inability to procure a fair trial for the application of the boiling water in this case, as I certainly think, from the appearance of the parts from which the desquamations took place from its use, that had the limb been kept in the water, until the action of the exhalant vessels subjacent to the morbid integuments had been thoroughly excited, the separation of the latter would have been effected, and that rest in the horizontal position, alterative aperients, mercurial dressing, and moderate bandaging, would probably have completed a cure.

Of the *dropsical* cases, one may be recorded. A man, aged 38, who for several years had been given to excess in spirituous potations, was induced by the consideration of a large helpless family, to make a solemn promise of abstinence for twelve months. About one month after this, his health, which had hitherto been very good, evidently began to decline, and symptoms of ascites to supervene. In spite of all remedial measures, the abdomen quickly became so enormously distended as to demand paracentesis. The operation afforded but a very temporary relief, as its repetition became necessary in a very short time, and suffice it to say, that the same operation was had recourse to seven times previous to the expiration of the year which had been dedicated to temperance. The time having now arrived when his anti-spirituous vow became null and void, I was as anxious as was my patient to try the effects of his former beloved beverage. Whiskey was accordingly ordered (and not without being taken) to the extent of two glasses daily, soon increased to three, when every symptom of dropsy, as well as of general decline, speedily disappeared. He is now in perfect health, and following his occupation, that of a stone

mason. I need scarcely say, that he still adheres to this regimen.

Laceration of the perineum seems to be of more frequent occurrence than is generally supposed. Four bad cases of this accident were met with, trifling cases being probably unheeded. Midwives of course were the attendants on these occasions, who in this neighbourhood seem so peculiarly tender in their mercies, that in place of being so cruel as to offer any obstruction to the descent of the head, by making pressure on the perineum during the accession of a pain, they on the contrary, at that time, at least in tedious cases, seldom neglect "to make room for the child," by forcibly pulling asunder the labia! Rest, cleanliness, and keeping the parts in a state of approximation, effected as much in these cases as could be expected.

A considerable number of *casualties*, and several cases requiring operations, were presented. As this paper, however, has already grown too long, I shall very briefly mention the particulars of two or three of these cases.

A man received the stab of a sharp-pointed instrument, which *wounded the brachial artery*, about a hand's breadth above the elbow joint. The blood gushed out *per saltum*, and a large quantity was lost in a very short time. The wound being small, the hæmorrhage seemingly of easy arrest, and having a few years back treated a similar case with the best result by pressure, I resolved to resort to the same means in the present instance. A graduated compass was accordingly applied over the wound, and the limb from this to the fingers pretty firmly enveloped by a bandage. A short distance above the wound, another compress was adjusted over the brachial artery, for the purpose of impeding the current, but was made to project considerably above the surface of the integuments, so as that the bandage which retained it would admit of a free collateral circulation. The man was kept in bed, lived on slops, and took digitalis for eight or ten days, when the wound

was perfectly healed, and the artery apparently in its natural state.

A young man received a *gun-shot wound* in the ham. On visiting him a few hours afterwards, I found the foot and leg cold, and without any appreciable pulsation ; no hæmorrhage of consequence had taken place. Believing the popliteal artery to be wounded, and amputation of the limb necessary, I immediately sent for further advice. A medical friend soon arrived, but whose opinion of the case did not coincide with mine. He urged the propriety of attempting to save the limb. Mortification manifested itself on the foot in a few days, and very soon after became master of the leg. The patient's strength was now rapidly failing, his pulse 130, and like a thread, his countenance almost cadaverous, and the *disease quickly extending upwards*. As a *dernier* resort, amputation was now decided on at a numerous consultation. I accordingly removed the limb about six inches above the knee, though in the full expectation of finding the man dead the following morning. The next day, however, it was found he had passed a good night, felt and looked better, and from this date he gradually recovered.

On examining the limb, the popliteal artery was found completely lacerated, the soft parts below the knee were sphacelated, and those above it, almost as high as the place of incision, injected with a greenish serum.

I think it will be admitted, that a stronger proof than this case could not possibly be given, of the occasional propriety of amputation in spreading gangrene.

Two cases of *ectropium*, both occurring after deep burns, were operated on with complete success, by freely dividing the cicatrices over the eye, keeping the lips of the wound well dilated with prepared sponge, till the granulations which spring up were sufficient to effect the same purpose, and by excising a portion of the internal membrane of the lid, and touching

the abraded surface every two or three days with nitrate of silver.

A woman fell on her elbow, and *fractured her olecranon* : severe pain, with a sense of numbness, was felt along the ulnar nerve and its branches. The bone was put in its place, and there retained in the usual manner, but the pain and numbness still continued. There was no swelling, heat, nor extra-uneasiness, on pressure, along the course of the ulnar nerve. The pain continuing undiminished, after the lapse of some days, (the numb sensation had now subsided), it was thought the olecranon might be effecting some undue pressure ; the bandages were, accordingly, unloosed, the olecranon carefully examined, and moved in different directions, but without any relief. Leeches, anodyne fomentations, anodyne liniments, and draughts, &c., were now made use of for two or three weeks, but without the pain being alleviated. A dozen of needles were now inserted deeply along the course of the ulnar nerve, and allowed to remain two hours : the pain was considerably relieved. The next day the needles were again inserted, when the amendment was very remarkable. In three days after this, the pain in some measure returning, the same remedy was had recourse to, after which the uneasiness rapidly and permanently subsided.

From the numbness with which the ulnar nerve was for some time affected, subsequent to the accident, as well as from its exemption from the ordinary indications of inflammation, and the manifest relief which so plainly followed the acupuncture, I think this case goes far to support the theory promulgated in one of the late numbers of this Journal by Doctor Osborne, namely, that “neuralgia is nothing else than pain arising from paralysis of the nerve, sufficient to alter its mode of sensation, but not so complete as to obliterate it.”

ART. XII.—*On Cirrhosis of the Lung.*—By D. J. CORRIGAN, M. D., Lecturer on the Theory and Practice of Medicine ; Physician to Jervis-street Hospital, and to the Fever Hospital, Cork-street ; Attending Physician to the R. C. College, Maynooth ; one of the Vice-Presidents of the Dublin Medico-Chirurgical Society, &c.

[Read at the Evening Meeting of the College of Physicians in the College Hall, on the 19th March, 1838.]

THIS disease, in its symptoms and signs, bears the closest similarity to phthisis. Its formation exemplifies a law of diseased action, which extends through many organs of the body. An account of it will, therefore, I hope, be found interesting both to the pathologist and practical physician.

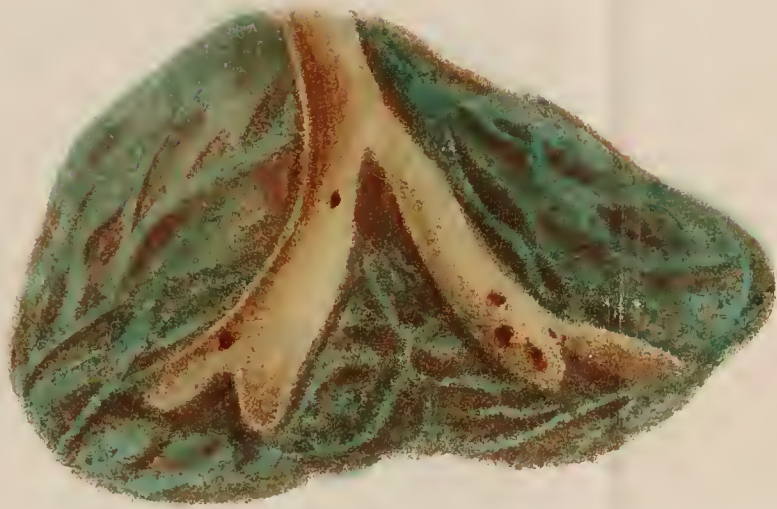
This disease is in the lung what Cirrhosis is in the liver ; and I have, therefore, ventured to call it by the same name.* The name of Cirrhosis has been given by Laennec to a peculiar disease of the liver, of which the pathological characters are : a diminution in bulk of the organ, and its natural acini or lobules deposited in the meshes of a dense fibro-cellular web, the fibres of which radiate in every direction through the organ.

This form of disease was supposed by Laennec to depend on the deposition of a morbid substance in the tissue of the liver. But later and more accurate examination,† particularly with the microscope, has proved beyond a doubt, that those granulations, which were supposed by Laennec to be new struc-

* A better name might be selected, but as there are already in medicine so many instances of names of diseases bearing no connexion with their nature, and as the name of *cirrhosis* is universally retained for the analogous morbid action in the liver, although it only expresses an accidental quality of the disease, I have thought it better to retain the name, than burthen our nomenclature with another. I would rather add an additional fact than a new name to our science.

† Vid. Cruveilhier Anatomie Pathologique, Art. Cirrhose.

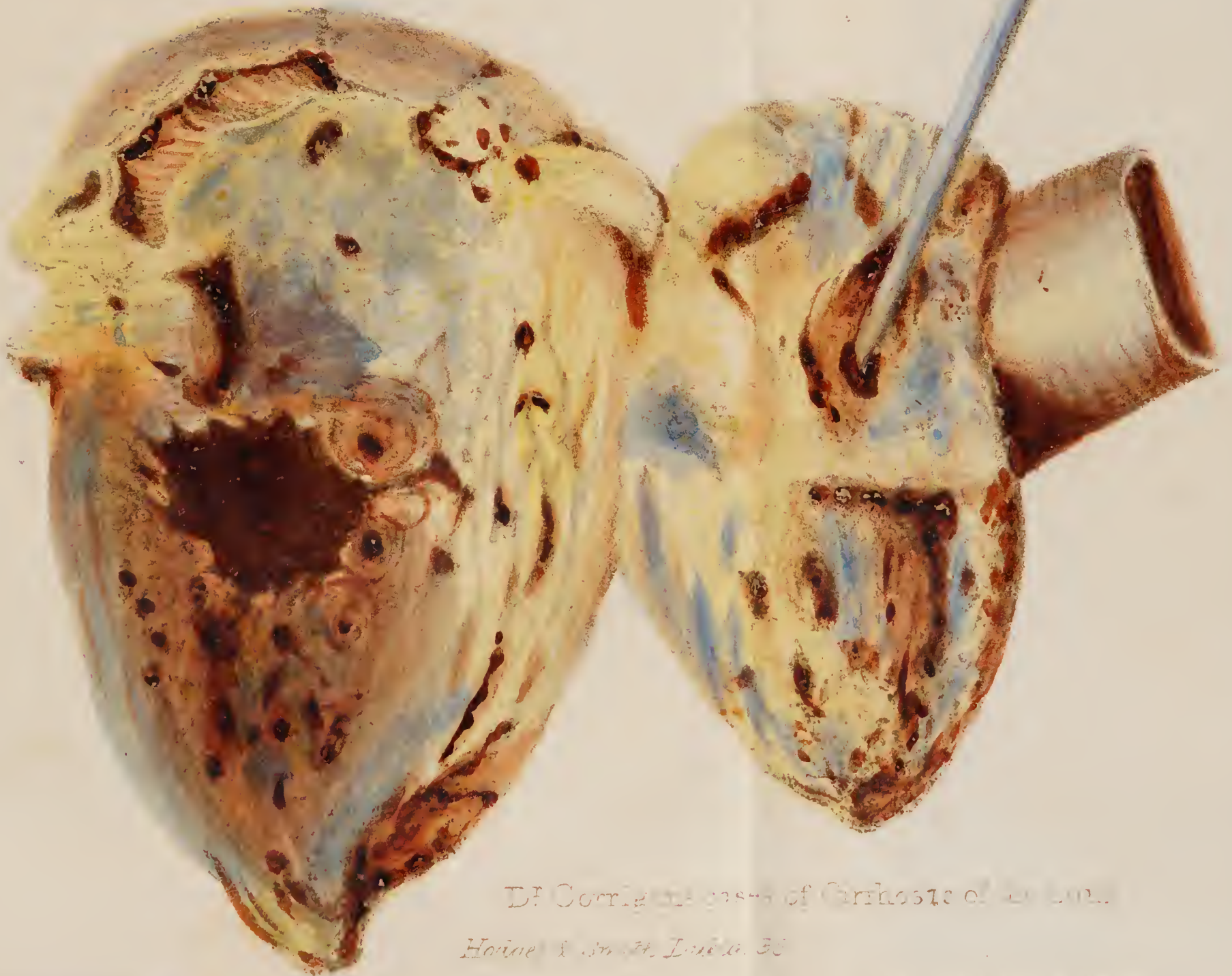
Fig 1



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a.

Fig 2



Dr Corrigan's case of Cirrhosis of the Liver.

Hodges & Smith, Dublin, 38



ture, are the natural secreting lobules of the organ. It is impossible, I think, to examine, with any degree of attention, this peculiar disease of the liver without recognizing its nature ; without coming to the conclusion, that the primary disease is in the fibro-cellular web which supports and surrounds the secreting acini. Commencing often without any distinct point of time to which to refer its origin, growing without pain, and at length only revealing itself in the living body by the production of one of its consequences, dropsy, its appearance, on examination after death, coincides with these symptoms. The fibro-cellular tissue, which constitutes the matrix for the vascular and secreting granules of the organ, has been gradually contracting itself. The fibres constituting this web, in contracting, necessarily draw in the organ from all points of its circumference. The liver, as this process of contraction proceeds, necessarily grows smaller and smaller ; its vessels are obliterated ; the fibres of the cellular web, as they shorten, necessarily thicken, and become more plainly marked ; and at last, when the disease has reached its last stage, the organ, drawn in by this slow contraction of its cellular tissue, is reduced from its very large bulk often to less than one-fourth of its natural size, while it has, perhaps, lost all trace of its natural shape, and a section of it shews a marbled appearance, arising from the veins of dense white fibro-cellular tissue running in every direction, among and between the yellow granules of the organ, tinged deeply of this colour by the bile which has been arrested in them.*

* The mistake committed by Laennec, as to the pathology of this disease, has been corrected by Dr. Carswell in his splendid work on pathological anatomy, art. Atrophy. The following is Dr. Carswell's account of Cirrhosis :

“ The production of atrophy from the development of the contractile fibrous tissue in the interior of organs, is no where so remarkable as in the liver, although it is occasionally observed, in a slight degree, in some other organs. The liver, when affected with atrophy from this cause, is sometimes reduced to a fourth of its normal dimensions ; its consistence generally increases with the diminution

The disease of the lung, which is the subject of this paper, resembles in every respect this morbid change in the liver, modified, however, by the situation and differing nature of the organ. Like the liver, the lung, in addition to its interlobular cellular tissue, has, for the matrix of its vessels and air tubes, a cellular tissue, the fibres of which permeate the tissue of the lung in every direction. The lung, in addition to this, is enveloped in a fibrous or fibro-cellular envelope, which has been described by Dr. Stokes.

There is still another structure in the lung, the action of which assists the growth of the disease ; it is the elastic tissue lying behind the muscular tissue of the trachea and bronchial tubes, and which, running in longitudinal fibres along these tubes, tends constantly to shorten them, and to draw all portions of the lung from the circumference towards its root. We see the action of this tissue in the living or dead body on an opening being made into the thorax, when we observe that the lung

of its bulk ; it appears shrunk, and has an irregularly rounded form, particularly at its edges ; and the whole of its external surface is raised into round flat projections, varying from the size of a hempseed to that of a pea, or even a small cherry. Examined more narrowly, the round flat projections are found to be composed of several smaller ones, and these, again, of the individual lobules of the liver, so that the larger projections are formed of aggregated groups of lobules, each separated, the one from the other, by cellulo-fibrous or fibrous tissue, the quantity of which varies considerably, and is always greatest between the largest groups of lobules. The interior of each group of lobules, when exposed by section, presents a number of fibrous intersections, continuous with the common capsule, and are obviously formed by this tissue, where it surrounds the terminal divisions of the portal veins. This tissue must, from its locality, and in virtue of its contractile property, tend continually to diminish the capacity of the vascular structure of the liver, and consequently, its entire bulk. It is, perhaps, not unworthy of remark, that the tuberiform aspect of the surface of the liver is to be ascribed entirely to the contractile property of the fibrous tissue, for it has been supposed that it was produced by the development of a new tissue of a peculiar kind, which was considered by Laennec to constitute the disease to which he gave the name of *cirrrose*, on account of the rust-brown colour which it so frequently presents."

does not fall flat or collapse, but instantly, by the elastic action of this tissue, draws in its circumference in all directions towards its fixed point, the root. The structure of the lung, its fibrous covering, its great quantity of strong cellular tissue, and the elastic lining of its tubes, all tend to give it a liability to the process of contraction in its cellular tissue, which constitutes the disease, which I have ventured, from its analogy with a similar disease in the liver, to name *Cirrhosis of the lung*.

While the primary pathological action in liver and lung is thus identical, the consequences must vary. Circumstanced as the liver is ; the soft and yielding organs about it, and the abdominal parietes around, present no obstacle to the contraction which is going on, and hence the whole tissue of the organ, veins, arteries, and ducts, becomes equally compressed. The space gradually deserted by the contracting liver, is filled up by the fitting in of the yielding organs around. But the lung is not so circumstanced. As the process of diseased contraction goes on, the space that is abandoned by the receding lung cannot be thus filled up ; the space that the contracting lung is deserting is therefore pressed in upon, or filled up in another way. In proportion as the contraction of the fibres of the fibro-cellular tissue obliterates the small air-vesicles, and as these contracting fibres, like so many elastic strings extending from the root in all directions, tend to contract or draw in the tissue of the lung, obliterating its small air-tubes and its blood-vessels, the large bronchial tubes dilate to supply the place thus left, until at last, when the disease has reached its last stage, the tissue of the lung, diminished to a very small size, presents no longer any permeable air-vesicles, but a dense fibro-cellular or fibro-cartilaginous tissue, with its fibres radiating in every direction through second and third sized bronchial tubes dilated into cells or ending in culs de sac of every variety of size. In this state a section of the lung through these dilated and tortuous tubes resembles very much a section of those honey-combed stones which we find on the sea-shore, the work of the

Pholas. This dilatation of the bronchial tubes is partly owing to the contractile process going on in the tissue of the lung, partly to the expansive action of the parietes of the chest in the acts of inspiration. The mode in which the diseased contractile action going forward in the cellular tissue of the parenchyma of the lung produces dilatation of the bronchial tubes, is, I believe, easily explained. If there were but one bronchial tube with contracting fibro-cellular tissue placed around it, then the contracting tissue would, as in the instance of stricture of the œsophagus or rectum, cause narrowing of the tube ; but when there is, as in the lung, a number of bronchial tubes, and the contracting tissue not placed around the tubes, but occupying the intervals between the tubes, then the slow contraction of this tissue will tend to draw the parietes of one tube toward the parietes of another, and necessarily will dilate them.* This dilatation is still farther favoured by each expansive act of the chest in inspiration tending also to draw the sides of those tubes asunder ; for, as the lung must follow the parietes of the chest in the expansion of inspiration, and as the vesicular texture of the lung cannot expand in the same proportion, the expansive action of the thorax in inspiration will necessarily draw asunder the sides of the bronchial tubes. Thus the diseased action and the natural function of the organ equally tend to the same effect, dilatation of these tubes. But as the contraction of the fibro-cellular tissue proceeds, other changes take place. The bony parietes of the chest begin to follow the receding lung, so that the side of the chest in which

* If the fingers of the reader's hand, separated one from another, be supposed to represent a bunch of bronchial tubes, and that the intervals be supposed to be filled up with fibres undergoing a slow contraction, it is obvious that the first tendency of that contractile action will be to draw the sides of the fingers towards one another, or if they were tubes, to dilate them. It is probable that the ultimate result of the contractile action, going on unimpeded for a long time, would be to draw in the whole lung so closely to the root, as even to obliterate those tubes which had previously been dilated ; but I am not able, as yet, to speak positively on this point.

the diseased action is going on, falls in, and when the disease is in the right side, the heart follows the receding lung, so that instead of being felt in its natural position, it is gradually drawn over to the right side, and is felt beating under the right mamma. The liver is from the same cause drawn up remarkably high into the chest, and the left lung becomes greatly enlarged, passing over into the right side of the thorax, and sometimes attaining an increase of bulk of more than one-half of its natural size. When the left side is affected, the heart turns round until its apex is in a direct line under the axilla, or even still more posteriorly situated. The appearance of the lung necessarily varies according to the degree which the diseased contractile action has attained. I have had an opportunity of observing it in its early stage. The lung presents much of its natural appearance, but it is denser and darker than natural, not brittle, but feeling to the finger like compressed or carnified lung, while radiating in every direction through it, are dense, firm, white fibres, which are the filaments of the fibro-cellular tissue becoming dense, firm, and white, by the contractile process going forward in it. The second and third rate bronchial tubes, instead of growing smaller as they proceed in their course, are dilated, and end in culs de sac, while on the sides of these culs de sac are seen the orifices of the still smaller branches; these smaller branches, on examination, are found to be imperious. The duration of this disease is generally slow. In the case which furnished me with the most recent preparation of the disease, the illness was of three months' duration, while in another case a period of twenty years had elapsed from the date to which the patient referred the commencement of his illness. The same uncertainty of duration attends the analogous disease in the liver.

I shall now relate a few cases of the disease. The first shews the diseased action in an early stage. For it I am indebted to the kindness of my friend Dr. Hunt. The patient whose death afforded this opportunity of seeing this disease in

its early stage, was cut off by an acute attack. The affection of which we are at present speaking, had little or no influence in causing death.

CASE.—John Chambers, æt. 7, was admitted into Jervis-street Hospital, May 17, 1837, with severe cough, dyspnœa, and hurried respiration, with high fever, and pulse 120. Three months previously, he had been attacked by influenza. The cough and expectoration continued ; he lost flesh, and occasionally spit up blood. Nine days before admission, the acute attack for which he was received into hospital, set in.

The right side of the chest gave a dull sound on percussion, and was perceptibly flattened in. Over it there was bronchial respiration, with distinct broncophony. The acute attack rapidly increased in severity, and he died six days after admission into hospital.

Post Mortem.—There were no tubercles in either lung. The left lung throughout was healthy. There was slight pleuritis of the right lung ; its tissue was solid to the touch ; of a greyish red colour, and devoid of crepitation ; but instead of being brittle, it was tough to the finger, and when cut into, it was found traversed in every direction by thick white bands of fibro-cellular tissue. The bronchial tubes, instead of growing smaller as they ran towards their termination, increased in size, and continued enlarged, with their lining membrane of a dark red colour, until they almost reached the pleura, terminating in oval or rounded cavities. In some of these oval or rounded dilations, the openings of the smaller bronchia were seen crowded together, giving to them an appearance similar to that of the bronchia of the tortoise. The smaller bronchia were not permeable beyond their orifices.

Fig. 1 shews the appearance of a section of this lung, the radiating fibres of the contractile fibro-cellular tissue of a whitish colour, running in every direction through the dark-coloured tissue of the lung, and the dilated bronchial tubes, A A, terminating in culs de sac.

The next case gives us an example of the disease in its most confirmed state, when it has reached the last stage.

CASE II.—B. F., a woman, about thirty years of age, with the following symptoms, sought for advice. Her respiration was very laborious, and she could only breathe in a semi-erect posture ; but there was no lividity of the face. She could not lie on the left side ; her cough was very troublesome, with viscid and purulent expectoration ; her pulse was about 100, very weak ; she was dropsical in the lower limbs and abdomen, and there was thirst, vomiting, and diarrhoea. The physical examination of the chest afforded the following results :—the right side of the chest gave throughout a remarkably dull sound on percussion ; over the anterior and superior portion of this side there was gargouillement, with cavernous respiration, cavernous cough, and pectoriloquy. The same signs, but not so distinctly marked, existed posteriorly. But what attracted most attention was the displacement of the heart ; the impulse of the heart was felt and seen not in its natural position in the left side, but in the right side under the right mamma.

The left side of the chest sounded generally throughout, clear on percussion, but less clear in its anterior superior portion than inferiorly. In its superior portion there was loud mucous rattle, with some resonance of the voice. The patient at the time of this examination was in such a debilitated state, that I could not obtain as satisfactory a history as I wished for of the progress of her illness. I learned, however, that she had for some years been complaining of her chest and of inability to lie on her left side ; but her illness had not assumed a very serious aspect until about six months previously, when she was attacked by spitting of blood, accompanied with palpitation of the heart. She lived only a few days after being seen with the symptoms above described.

On examination after death, the right lung was found reduced to the size of about two fists. It occupied the superior portion of that side of the thorax, and was intimately adherent

to the pleura costalis. The remaining portion of this thoracic cavity was filled up by the heart, which was found lying to the right of the sternum, and by the liver, which ascended unusually high into the chest. On making a section of the right lung it was found to have lost every appearance of healthy lung; it consisted of a confused congeries of large bronchial tubes, united together by a dense firm tissue, without any trace of a tubercular cavity having ever existed in it. The bronchial tubes, which terminated abruptly in rounded cavities, were filled with muco-purulent matter. The left lung was enlarged by at least one-half its usual size; in its centre were four or five small abscesses, and in the inferior portion were small portions in the state of red and grey hepatization.

Fig. 2 is a drawing of the lung of full size in this case, shewing cirrhosis of the lung in its confirmed state. It shews what an extreme degree of contraction the lung had undergone; the mass of the lung consisted of dense, whitish, firm, fibro-cellular tissue, marbled here and there by the remains of the natural colouring matter of the lung. B B B are the dilated culs de sac of the bronchial tubes. The space left by this extreme contraction of the lung was filled up, as already observed, by the displaced heart, by the increased development of the opposite lung, and by the protrusion upwards of the liver.

To the practical physician this case is peculiarly interesting, from its extremely close simulation of phthisis. It presented every one of the physical signs of phthisis; the right side was quite dull on percussion; there was in it gargouillement, cavernous cough and respiration, and pectoriloquy. In truth, were we to be asked for a collection of those signs, which could most unequivocally be depended on as certain evidence of consumption in its last stage, the signs found in this very case would be the signs named. The resemblance to phthisis was still further increased by the attacks of spitting of blood. There was, as already observed, however, not a single tubercle in the lung, but the state of it, as found after death, adequately ex-

plained the signs that had existed during life. The lung was solid in its tissue, which gave the dulness of sound on percussion, while the culs de sac of the dilated bronchial tubes, filled with fluid and air, furnished the gargouillement, cavernous cough, and pectoriloquy. It might, perhaps, be supposed, at first sight, that in this case the small size of the lung might have been the consequence of an attack of pleuritis, with effusion, in which, after the effusion had been absorbed, the lung had never expanded; but this supposition will not bear the test of examination, either by the history of the case, or what is still more satisfactory, by the pathological appearances. Nothing can prevent the unfolding or dilatation of a lung, after the removal of fluid which had compressed it, except the deposition around it of lymph of a fibro-cartilaginous nature; and Laennec, vol. ii. p. 340, has already observed, that in all cases where the chest has been much contracted in the cure of pleuritis, the deposition around the lung has been fibro-cartilaginous. There was none of this deposition around the lung in this case; the adhesion between the pleura pulmonalis and costalis was by fine cellular tissue, which could not have interfered with the re-expansion of the lung; and indeed the appearance of this lung, with its firm, whitish, fibro-cartilaginous texture, is so dissimilar to that of the fleshy, dark blue, carnified lung, that, under an eye accustomed to the pathological alterations of the pulmonary tissue, they could not for an instant be confounded together. There is, moreover, this distinction between the lung compressed by pleuritic effusion and the change here described, that in the former, no matter to how small a size the lung may be compressed by effusion, (provided its structure be not affected by extension of the diseased action), its vesicular texture is preserved, and is capable again of unfolding itself, while, in the disease here described, the vesicular texture is totally destroyed. I have now before me, while writing, for the purpose of contrasting them, a preparation of Cirrhosis of the lung, and a lung compressed by effusion; in the latter, which is reduced to

the size of an orange, from great and long continued pressure* the vesicular texture is still quite visible, while, in the former, not a trace of the texture is to be seen.

The next case of dissection of Cirrhosis, I have found in Laennec. The appearances coincide very closely with those just related.

CASE III. (*from Laennec*, vol. i. page 216.)—A man was admitted into hospital in the winter of 1821-2. For a period of twenty years he had been suffering from cough and muco-purulent expectoration. He laboured under oppression of his breathing. The left side of the chest was by one-third smaller than the right, and over the inferior angle of the left scapula there was marked broncophony. A few hours after his admission into hospital, he died suddenly, with symptoms of apoplexy.

The left lung was not larger than the size of two fists; it was adherent nearly throughout to the pleura costalis. The whole tissue of this lung was transformed into a substance of a fibro-cartilaginous appearance. The bronchial tubes were nearly all dilated; their diameters, from their first division to their terminations in culs de sac, not varying more than from two to three lines. The smaller ramifications were obliterated. The right lung was sound, and very large.

It is singular in this instance, that while every thing most characteristic of the disease was present, the fibro-cartilaginous structure of the lung, the obliteration of the air-vesicles, the termination of the bronchial tubes in culs de sac, the falling in of

* In the case from which the preparation here alluded to was obtained, the attack of pleuritis terminating in empyema, occurred in January. The operation for empyema was performed by Mr. Adams in September following; the lung is reduced to the size of an orange; it was compressed by a large quantity of purulent matter, and layers of lymph at least two inches thick. Could pressure alone obliterate the vesicular texture of the lung, there should not have been a trace of it preserved here. In some cases of pleuritis the diseased action may extend along the interlobular cellular tissue of the lung, and then the result will be the combination of Cirrhosis with pleuritis, which will tend powerfully to increase the contraction of the side, and to cause, in the manner already described, dilatation of the bronchial tubes.

the affected side, and the immense development of the opposite lung ; the true nature of the disease, or the connexion between it and its peculiar signs, seems never to have suggested itself to him, and he only notices the case as giving an example of dilated bronchial tubes.

In the two following instances the cases are still under my observation.

CASE IV.—Mr. T., æt. 47, for four years has suffered frequently from dyspnœa. For the last six months, the dyspnœa and cough, with muco-purulent expectoration, have become more troublesome, and he lost flesh ; he has occasionally spit blood ; he can lie on both sides ; the pulse is 80, soft ; skin cool, and there are no sweats ; the action of the heart is regular ; the right side of the chest, the whole way down, but more particularly from the clavicle to the mamma, is flattened in ; this whole side sounds slightly duller on percussion than the left ; over it the natural respiratory murmur is very weak, and is replaced by a soft, low, mucous rattle. There is no bronchial respiration ; but there is resonance of the voice over the whole of this side. The left side is clear on percussion, and through it there is puerile respiration.

The remaining case of the disease, I have had had now immediately under my eye for some months. This patient has been in hospital, under Dr. Hunt's care and mine, since last December, and at different intervals from the period of admission up to the present the case has been examined by the Surgeon-General, Dr. Croker President of the King and Queen's College of Physicians, Dr. Marsh, Dr. Graves, who have authorized me to express their concurrence in the views which I have here offered of the nature and diagnosis of this disease.

CASE V.—Andrew Summerville, æt. 35, carpenter, was admitted into Jervis-street Hospital, 12th Dec., 1837. About twelve months since, he was attacked by severe cough, accompanied with spitting of blood, which continued for three months. There was then partial recovery for seven or eight months, followed again by cough and spitting of blood. The spitting of

blood has returned at intervals of a fortnight's duration, and with occasional sweats. On admission, his pulse was weak and slow, ranging from 60 to 72. The cough was very troublesome, accompanied with muco-purulent expectoration, and he could not lie on the left side. On stripping him for physical examination of the chest, there is a visible difference between the two sides of the chest. The right side scarcely moves in respiration, while the motion of the left is remarkably free. The right side of the chest is considerably flattened in, and smaller than the left, but it is not shortened from above downwards, nor is the shoulder depressed. The difference between the sides is most plainly seen by looking down on them from over the clavicle. There is slight dulness on percussion over the anterior and posterior portion of the right lung, and great resonance of the voice in the axillary region and posterior portion of same side ; there is over the whole of this side a great diminution of the natural respiration, and the impulse of the heart, instead of being felt in its natural position, is felt, although indistinctly, under the right mamma. Over the whole of the left side, including the cardiac region, there is clearness of sound on percussion and puerile respiration.

Since the date of the above notes of the case taken in December, 1837, the signs have grown more distinct, and at the present date, April, 1838, the resonance of the voice is greater in the right side, the contraction more marked, and the impulse of the heart is now distinct even to the eye under the right mamma. The general symptoms have scarcely varied ; the pulse does not range above 70, and there is no emaciation of any account.

In both this case and the case of the female, B. F., there was difficulty of lying on the sound side, probably for this reason, that to supply the function of the affected lung, the other side of the chest acted through a greater space ; and any circumstance, such as lying on the sound side, which would embarrass that free action, was productive of so much distress, that the patient dare not persevere in it. This last case of Somerville also answers a question which Andral has put in his Clinique

Medicale to be solved, viz., Why, in lung in which there were dilated bronchial tubes, the respiratory murmur or bronchial respiration was not present?* The affected side of the chest moved but little, or not at all, and hence there was no traversing current of air through the dilated tubes to produce sound.

The dilated bronchial tubes, such as I have described above, are described by Laennec and Andral, but the explanation of their connexion with the peculiar state of the lung, which I have ventured to offer, is totally different from theirs. Laennec attributes the dilatation of the bronchial tubes (p. 201, vol. i.) to masses of mucus lying in the tubes, which, not being expectorated, mechanically dilate these tubes. This explanation is quite insufficient. Masses of mucus, sufficiently firm to mechanically dilate bronchial tubes, and compress the surrounding pulmonary tissue, are never met in the bronchial tubes.

Andral, in his *Clinique Medicale*, vol. ii. p. 200, attributes the dilatation of the bronchial tubes to hypertrophy of the parietes of the bronchia, but how this hypertrophy produces dilatation of the tubes, and compression of the inter-bronchial pulmonary tissue, he does not explain, and I certainly cannot comprehend. Indeed the objection appears to have struck himself, that his explanation is insufficient; for the bronchial tubes, as he himself describes them, are in some instances, instead of being thicker, far thinner and weaker than natural; and in those instances, to explain their dilatation, he is forced to return to Laennec's explanation of distention by mucus, (p. 207).

Of this peculiar diseased action in the matrix of the lung, consisting of a gradual but certain contraction of the fibres of the cellular tissue, pathology furnishes us with many instances; but why fibro or fibro-cellular tissue should take in this contractile action is, as yet, a mystery. It is probable, that in a great number of such instances, the commencement is a slow inflammatory action, causing some deposition of lymph in the tissue

* Cl. Med., vol. ii. p. 202.

affected. This lymph then takes on the nature and properties of the tissue which has secreted it ; the diseased action does not, however, stop at this point, but the tissue affected, taking on a contractile action, changes, and symptoms are then produced, depending on the functions, situation, &c., of the organ attacked.

We see one of the most remarkable instances of this contractile action in the healing of superficial burns about the neck, where there is a quantity of this fibro-cellular tissue : lymph is effused, granulations are formed, and healing rapidly sets in ; but, unfortunately, a contractile action is set up, the fibres of the cellular tissue and skin shorten in, the skin is dragged or puckered from all around towards the part in which the diseased action is set up, and sometimes hideous deformity is the result. Dr. Carswell has seen mouth and nose completely closed up by this contractile action after a burn ; and when the skin about the axilla, or one of the joints, has suffered, a contracted and useless limb is too often the result.

In stricture of the intestinal canal, of the œsophagus, of the urethra, we see examples of the same diseased contractile action. Lymph is effused into the sub-mucous cellular tissue ; this tissue then takes on a slow contractile action, which eventually constricts the canal, and renders it impervious. In the urethra, the œsophagus, and the rectum, it can be kept at bay by mechanical means ; in other organs, it is out of reach by direct applications, and in some instances it proceeds in its course uncontrolled. In narrowing of the auriculo-ventricular opening in the heart, there is an example of the progress of this uncontrolled diseased contractile action. Lymph is shed upon or between the folds of the mitral valve ; this contractile action is set up ; it goes slowly and treacherously on. The edge of the curtain-like mitral valve is gradually drawn in all round, as the mouth of a purse would be by a running string, until the size of the opening is reduced from a size freely admitting two fingers, to a size not larger than a crow-quill, and confirmed narrowing or stricture is

thus formed ; but the same action which produces stricture of a tube in one instance, may, according to circumstances, produce patency or dilatation of it in another. Thus, in the aorta, when a similar diseased action is set up in the semilunar valves, the contractile action necessarily acts between the points of attachment of the valve, and the result, then, is a drawing back of the valve to the wall of the aorta, keeping the mouth of the aorta permanently open, so that the same action, according to circumstances, produces opposite or contrary consequences ; in the auriculo-ventricular opening causing narrowing ; in the aorta causing permanent patency. In like manner, in a bronchial tube, and in the œsophagus, the results of the same action will be different. In the œsophagus, contraction of the fibro-cellular tissue around this tube will produce contraction of its calibre, the tissue, in contracting, necessarily drawing the sides of the tube together ; but in the lung, the fibres which are undergoing contraction, not being placed around the tubes, but passing in every direction through the intervals of the tubes, and from one tube to another, they will, in proportion as they undergo contraction, draw the parietes of one tube towards the parietes of another, and necessarily widen these tubes. The most beautiful exemplification, however, of this curious contractile power, is to be seen in that extraordinary phenomenon, spontaneous amputation of the limbs of the foetus in utero, for the first accurate account and explanation of which, we are indebted to Dr. Montgomery.* In this curious phenomenon, Dr. Montgomery has shewn, that the efficient cause of the amputation is the formation of strings of lymph around the limb. Both surfaces of the amputated portions have been found healed,† that is, the extremity of the

* Vid. Dublin Journal of Medical Science, vol. i. p. 140, "Observations on the Spontaneous Amputation of the Limbs of the Fœtus in Utero, &c., by W. F. Montgomery, M.D., M.R.I.A., Professor of Midwifery to the King and Queen's College of Physicians in Ireland."

† Vid. Lancet, January, 1838, p. 642.

stump of the foetus, and the surface of the foot or leg which has been lying loose in the womb. I believe that in those cases the string of lymph, around the limb, has taken on this slow contractile action, and thus it has at last produced removal of the soft limb of the foetus by its gradual tightening, just as the surgeon, by gradually tightening a ligature, effects the removal of a polypus.

If this explanation, which I have ventured to offer, of the connexion of fibro-cartilaginous contraction of the lung, with dilated bronchial tubes, be correct, it is obvious that the dilatation of those tubes is to be regarded in the treatment of the disease not as the primary cause of the symptoms, which we are called upon to remove, but as the effect of that peculiar diseased action of the fibro-cellular matrix of the lung, to which I have ventured to give, from its analogy with a similar disease in the liver, the name of *Cirrhosis*.

On the diagnosis of this disease, a few observations may be necessary. When the chest is examined for the first time, in a case where the disease is in the right lung, the displacement of the heart, and the prominence of the left side, might raise a suspicion that the fulness of the left side, and the displacement of the heart, were caused by effusion into the left side of the chest; but a little more careful examination serves to remove the doubt, and to shew that the prominence of the left side is owing not to actual enlargement, but is only apparent, arising from the contraction which has been going on in the right side, while the clearness, on percussion, over the apparently prominent side, and the preservation of the natural form of the intercostal spaces, sufficiently prove that there is no effusion into the chest; and the natural sound of respiration equally proves that there is not pneumo-thorax of that side. But the disease, above all, that this affection most closely simulates, is phthisis: in all the cases which have come under my own observation, the similarity has been most striking. In the onset of the disease, the attacks of hæmoptysis followed, or alternated by purulent expectoration: as the disease becomes more and more formed, the combination

of these symptoms, and oppressed respiration, with dulness of percussion, broncophony, and pectoriloquy, and the gargouillement in the dilated tubes, altogether imitate phthisis so closely, that the diagnosis becomes a matter of great nicety. I can scarcely have a doubt that very many of those cases of phthisis which are recorded or noticed, as of very many years' standing, without any marked alteration for better or worse, are cases of this disease. I believe that in the commencement of the disease, the surest diagnostic signs will be found in the absence of the constitutional signs of phthisis ; and that if in a case resembling phthisis very closely, with hæmoptysis, broncophony, &c., we do not find corresponding constitutional irritation, that the pulse is not disturbed, that there is not wasting, in proportion to the amount of local signs, we may infer that the disease under consideration is Cirrhosis. There is, however, a circumstance relative to phthisis necessary to be recollected in making the diagnosis on these grounds ; it is this, that if there be any other organic disease present in the case we are examining, the *absence* of the constitutional symptoms of phthisis will not authorize us to infer that phthisis is not present. I never saw a case of phthisis where there was no other disease, where the usual attendant symptoms, quick pulse, emaciation, and hectic, either by diarrhoea, urine, or sweat, were absent ; but I have repeatedly seen cases where organic disease of some other organ either preceded or occurred in the progress of phthisis ; and in those instances the effect very often was to prevent or arrest the usual symptoms of phthisis. I attended a gentleman in phthisis, in whom, for some months, the symptoms and signs of phthisis, the emaciation, quick pulse, hectic, &c., were well marked : organic disease of the mesenteric glands supervened ; the hectic, quick pulse, &c., ceased while the signs remained. After some time, he died : the lungs were most extensively engaged in tubercular disease. I had another case under my care, where there was extensive abdominal disease : phthisis supervened ; the ordinary symptoms never developed themselves. It is not at all unusual to see cases of phthisis supervening, on organic disease of the

liver produced by intemperance, in which the same observation is applicable.

When Cirrhosis of the lung is fully formed, and that we have the signs in full perfection, there is then no longer any danger of confounding it with phthisis ; the very intensity of the local signs furnishes us with one of the diagnostic signs ; for while the local signs, such as dulness on percussion, pectoriloquy, gargouillement, and absence of respiration, would seem to point out rapid and confirmed phthisis, the non-accordance of constitutional disturbance in correspondence with such local signs, tells us that it is not to tubercular disease those signs are to be referred. If the disease be in the right side, the displacement of the heart into the contracted side gives us at once positive evidence of the nature of the disease.* In conjunction with these symptoms and signs, we have the history of the case, if it has been of long duration, to guide us ; for while the present signs are those of phthisis, there is again a non-accordance between the history of the case and those signs ; and while the signs indicate apparently confirmed phthisis, the history tells us that there has not been constitutional irritation or wasting corresponding with the local signs. One of the most curious circumstances connected with this disease, is the compensating power for the contraction of one lung, which is to be found in the proportionate development of the opposite lung. In one case, the sound lung was increased by one-third, in another by one-half of its natural size. The slower the progress of the disease is, the

* In examining the case of Summerville with me, Dr. Marsh pointed out to me another physical diagnostic sign between this disease and phthisis, arising from the direction of the line of demarcation between the clear and dull sound on percussion. In phthisis, where the dulness is from tubercular deposition, the dull sound under the clavicle extends to the sternum. In Summerville's case, the dull sound is bounded by a vertical line, proceeding downwards from the prominent arch of the clavicle, and corresponding with the points which the left lung has reached, in encroaching on the right cavity of the thorax, so that the vertical inner or sternal half of the right side is clear on percussion, while the corresponding outer half is remarkably dull.

more certainly will this compensatory development take place. The time occupied before the contractile action has reached its last stage in the lung, is very variable, just as it is in other parts where a similar action is taking place. We see this contractile action after burns in strictures, &c., in cirrhosis of the liver, sometimes reach an extreme degree in a few weeks, while in other instances, years pass over with very little aggravation.

On the treatment of this disease I have as yet but few observations to make. The disease is very rare; for although it has been the subject of my research for more than six years, I have been able to collect only those cases which I have related. Pathological preparations of the disease are not uncommon; but these are of no use, without being able to connect them with the details of the cases during life. In the limited experience that I have of treatment, I should say that one principal indication is to obviate the attacks of intermittent congestion that every now and then occur, as the process of contraction acts upon the vessels, and indirectly upon the sound lung; these are characterized by febrile exacerbations resembling hectic, by attacks of hæmoptysis, or by aggravation of the cough, accompanied by viscid expectoration; they are at once relieved by small general or by local bloodletting. In the intervals of these attacks, the relaxation consequent upon the profuse muco-purulent expectoration which often attends the disease, and the expectoration itself, are greatly relieved by decoct. senegæ with sulphate of quinine in solution in it. The quietness of the pulse, the relaxation, and the profuse discharge from the bronchial tubes, would often seem, when the circumstances above noticed are present, to render it unadvisable to leech or bleed, but the patient bears moderate depletion very well. Whether we have any remedial agent that can exercise such a power as to check the contractile action in cirrhosis of the lung or liver, remains yet to be discovered. If the disease be, as analogy would lead us to suppose, of a low inflammatory nature in its origin, it might be judicious not to leave the cure of inflammatory affection of the lungs to tartar emetic alone, which is now so gene-

rally the practice, but to finish the treatment by the cautious exhibition of mercury.* Whatever will favour the development of the corresponding lung, so as to enable it to do its own duty and also the office of its fellow, will, however, be the surest safeguard against the effects of the disease ; and with this view, it is probable that the most active life, as to exertion, particularly in such exercises as will tend to develop the sound lung, such as long walking, riding, gymnastic exercises in moderation, will be the most beneficial habits for the patient ; and above all, in the female, the discontinuance of the use of stays, or any dress that will press on the thorax and interfere with the compensatory development of the sound lung.

ART. XIII.—*Observations on Sciatica and other Neuralgic Affections, with a Consideration of their frequent Origin from a disordered State of the Stomach ; interspersed with some novel Views respecting the Origin and Prevention of Gall-Stones ; both Diseases exemplified by the Author's own Case ; together with an Account of the Waters of Bagnères de Bigorre and Barège in their Treatment.*
By RICHARD CARMICHAEL, M.R.I.A., Corresponding Member of the Royal Academy of Medicine of France, &c., and Consulting Surgeon of the Richmond Surgical Hospital, &c. &c.

[Read at the Evening Meeting of the College of Physicians, on the 18th of April, 1838.]

THE President having kindly invited me to read a paper at this meeting, I began to consider what subject would be most agree-

* Dr. Stokes, in his recent work on Diseases of the Chest, has noticed, and is the first writer who has done so, that contraction of the side has sometimes followed the cure of pneumonia. Contraction of the side after pneumonia is not usual ; and it is probable that, in the instance noticed, there was deposition of lymph into the interlobular cellular tissue of the lung, which gradually took on the contractile action, constituting the disease here described.

able to the College, when it struck me that some account of the Bagnères and Barége waters, to the salutary influence of which I feel indebted for the honour of now addressing you, might convey some information not generally known, and therefore be useful to those who may be induced to seek for that greatest of blessings, health, at the sources of these celebrated springs. This naturally led me into a statement of my own case, a most dangerous theme for any person to enter upon; for set a regular valetudinarian upon his favourite topic—the recital of his pains and aches—and he will proceed with undiminished ardour *ab ovo usque ad mala*, which may be appositely construed, from your egg at breakfast to your apple at supper.

With this fear before my eyes I shall endeavour to restrain *my* hobby within due bounds, and enter upon this perilous career with confidence, as I am persuaded that the suggestions which occur to a reflecting medical man concerning the disease under which he himself has suffered, are likely to throw considerable light on its true nature, and most appropriate mode of treatment: in support of which opinion I need only adduce the excellent Essay of Doctor Bree on Disordered Respiration, as well as many other works of medical men on the maladies with which they had themselves been afflicted.

My complaint was a most severe sciatica, preceded and accompanied with the various symptoms of dyspepsia, spasms of the stomach, and gall stones. It is necessary, in the history of my case, to state facts which shew how considerably derangement of the digestive organs is concerned in the production of sciatica and other neuralgic affections. In these observations I shall have occasion to refer frequently to the appearance and characters of the urine, as indicative of the state of the stomach. I shall also offer some conjectures respecting the cause of the formation of gall stones, and consequently how this most painful malady may possibly be prevented. With this notice duly entered, you may perceive, Mr. President, that the paper which I am about to read is a melange which treats *de omnibus rebus*

et quibusdam aliis; and that perhaps the recital of my own case ought only to be esteemed a convenient peg upon which to hang these various topics. Be that as it may, I shall endeavour to condense my observations into as narrow a compass, as the necessity of being intelligible will admit.

I attribute the general derangement of my health, which led to the attack of sciatica, to an error in regimen, to which all professional men are most prone—I mean to that inattention of taking in nourishment at regular and fixed periods; and this is an error most likely to occur when it is most desirable that regular supplies of nutriment should be taken, in order to restore the waste which great bodily or mental exertions occasion. Having reliance upon a good constitution capable of bearing great fatigue, I became habituated to fast from eight o'clock in the morning until eight in the evening. In October, 1827, after a particularly severe day's work, both mental and bodily, I was seized during the night with an acute pain in the region of the stomach, attended with a feeling of extreme weight and distention extending to the spine. This was attended with coldness of the extremities and a pale ghastly appearance of countenance. It had all the characters of an attack of spasm of the stomach, and lasted several hours, but at length yielded to hot water, brandy, and opium. Similar attacks occurred occasionally, but did not prevent me from attending to my professional business, which I performed, however, with a reluctance and *ennui*, by no means natural to me. In the November following I was attacked with a gastric fever of the most severe description, attended with incessant sickness and vomiting, pain on pressure in the region of the stomach, and delirium. These symptoms yielded with great difficulty, to repeated leeching and blistering over the region of the stomach; and to small doses of calomel frequently repeated. When sufficiently restored I went to the country to recover my strength; but had the imprudence to remain there but a fortnight, when I returned to Dublin, and resumed my professional practice, while I was still scarcely able

to walk up and down the stairs of the houses of my patients. I felt at this time an unusual languor and lowness of spirits, although at no period of my life was I more prosperous in my profession. My appearance betokened ill health, and I felt no inconsiderable apprehensions that I was about to become a victim to diabetes mellitus, as the urinary secretion had increased to upwards of sixteen pints daily, was colourless, and accompanied by a dry state of skin, while at the same time I had a ravenous appetite. On examination, however, the urine was found to be only albuminous, and did not exhibit any signs of saccharine matter. These alarming symptoms of approaching diabetes mellitus were obviated chiefly by the daily use of the warm bath, which restored the skin to its functions; and I was gratified in finding, that as the skin became soft and perspirable, the secretion of urine diminished.

I look upon a close observance of the state of the urine to be of the utmost importance to the dyspeptic patient. For not only is the deviation just noticed of consequence, but when he finds it vary from the clear, healthy, amber colour, and become dark, clouded, and turbid, or otherwise changed in ways that shall be adverted to, he may be certain that he has eaten or drunk more than was prudent; or, that the quality of his food was such as to irritate and excite his stomach beyond bounds, the powers of which had been already weakened; or that he has prolonged his fasting to too great an extent; or that he has used exercise, either mental or bodily, immediately after meals; or that he may have neglected to use sufficient exercise at the proper periods for bodily exertion; or finally, that the cares or anxieties of life have, through the medium of the brain and nervous system, disturbed the functions of this all-important organ. In fact, the state of the urine points out when errors are committed by the dyspeptic against the organic laws, with the same promptitude as the fabled ring of the Sultan Amurath indicated those against the moral laws; and these indications follow the errors in regimen above alluded to, with such constancy, that little mischief is likely to arise, if

they are received as a warning against their repetition. By this precaution alone, functional disorders are easily remedied, and organic changes most effectually prevented, not only in the stomach and intestinal canal, but in those which are likely to follow, if the first are unattended to, viz., in the liver, kidneys, lungs, and brain. *Venienti occurrere morbo*, is an adage, the wisdom of which every body admits, but to which few attend.

It may be imagined from these observations, that I wish to bring the profession back to the state it was when the urine doctors were held in high repute. This is by no means my object. Mankind are for ever prone to run into extremes, and the practical utility arising from the appearance of the urine was at one time in such estimation, that all the other symptoms, with which a patient was affected, were overlooked, in comparison with the indications afforded by the urine. Just in the same way as some young medical men of the present day seem to think, that in discriminating diseases of the chest, the sounds conveyed to the ear by that most useful instrument, the stethoscope, may supersede the necessity for inquiry into the history and general symptoms of the disease. This folly of the urinists, was pointedly, but not very delicately, ridiculed by the celebrated Doctor Radcliffe, Physician to Queen Anne. A woman brought him some urine in a vial, and presenting it, requested advice for her sick husband. What is your husband's business? A shoemaker, was the reply. Then tell him, quoth the doctor, to make me a pair of shoes, here is my measure; at the same time returning to her the vial, from which he had poured her husband's urine, and replaced it with his own. This well-known anecdote of the sarcastic Doctor Radcliffe, did more to upset the pretensions of the urinists than the most demonstrative argument. But surely, we should not on this account neglect the indications which the changes in the urine afford of functional derangement—the very threshold of disease. When diseases are fully formed, or organic affections exist, as in fever, gout, hepatitis, dropsy, &c. &c., the useful practical indications af-

forded by the state of the urine are universally admitted ; but I would wish to call attention to the practical advantages of also watching the state of the urine in every case of disorder of the chylopoietic viscera, particularly of the stomach. For several days the urine in dyspeptic persons may remain of a clear, healthy, amber colour, then it will suddenly become clouded, sometimes with a white, and sometimes with a yellow or nut-coloured brown sediment ; at other times it will be of a deep brick-red, on which occasions the urinal is often coated with a pink-coloured sediment, such as is found in the urine of gouty persons. Prout observes, respecting this deep colour, that “ the more the sediments approach to *brick-red*, the more of lithate and purpurate of soda they contain ;” and that this deep colour always denotes the presence of some feverish or inflammatory action. The white or yellow sediments consist of lithate of ammonia, with more or less of the phosphates ; and these are the sediments to which even the healthy are subject, when they commit excesses in regimen ; but considerably more so the slightly dyspeptic. In the latter they may be occasioned by the most trivial errors in diet. When the urine exhibits those coloured sediments, the lithic or animal acid diathesis may be said to prevail ; but in other instances in which the urine is pale and much increased in quantity, and deposits, when cool, white earthy-looking sediments, which consist of the triple phosphates of magnesia and ammonia, the uric acid seems to be deficient, and, therefore, not eliminated by the kidneys from the system. The urine in cases of this description does not redden litmus paper ; on the contrary, it is alkaline ; but this may be owing to the evolution of ammonia, as it is very prone to decomposition. This state of the urine is indicative of the highest degree of derangement of the digestive organs, in which not one of them seems to perform its allotted functions. There is sickness, flatulency, acidity, constipation, and black or clay-coloured faecal discharges. In this state of urine a pellicle often forms on its surface, which, we are informed by Prout, consists of minute

crystals, composed also of the triple phosphate of magnesia and ammonia. The expression of the countenance in such instances is pale, haggard, and betrays universal derangement of the system. This was my state at the period of presumed convalescence, which succeeded and continued for many weeks the gastric fever I have mentioned, and is the state which Prout terms the phosphatic diathesis. As I improved, it changed to the lithic diathesis, which continued until I drank the waters of Bagnères, of which I shall soon speak more fully. During this period of my illness, the slightest deviation from the strictest rules of diet was sure to be followed by sediments in the urine, whose depth of colour corresponded with the degree of irritation, or dyspeptic symptoms induced. I was obliged to relinquish vegetables altogether, and of animal food and bread I could not, perhaps, take one-fourth of what my appetite would prompt me to use, without exciting unpleasant symptoms, always followed by those sediments.

The attacks of spasm or sudden pain at the pit of the stomach, extending to the back also, became gradually more frequent. I now began to suspect that they were occasioned by gall stones, as my eyes and skin became tinged with bile after each successive attack, and the urine betrayed the usual appearance it presents in all jaundiced persons. The attack usually came on between two and three hours after a meal, the first indication of which was a sense of fulness at the pit of the stomach, the certain precursor of a most overcoming sensation (as if a heavy weight were lodged there) between this and the spine, that often made me writhe with agony upon the floor. Large and repeated doses of opium, with draughts of hot water, afforded the only relief to attacks, under which I continued to suffer subsequently for more than two years, and which necessarily occasioned great debility and emaciation.

One night, after a severe attack of this description, which came on as usual about two hours after eating, I began to consider why it always happened, that the attacks should so regu-

larly occur between two and three hours after a meal, no matter whether breakfast or dinner. The obstruction of a gall-stone in the duct, which occasions the pain, is mechanical, but why should it only occur at the periods mentioned? To this question the following answer suggested itself to my mind.—That as soon as the chyme passed from the stomach into the duodenum; (which was ascertained by Dr. Beaumont of New York, to be on an average of from two to three hours,) its presence in the intestine stimulates the extremity of the common gall-duct, and consequently the demand is answered by a supply of bile, to complete the process of chylication, not only from the liver but from the gall-bladder. The flow of bile from the latter carries with it the gall-stones it contains, if not too large to enter the cystic duct. These becoming impacted in some portion of the gall-ducts, occasion the spasms and severe agony which attend the paroxysm, until by the use of opium or other means, such relaxation of the spasms is induced, as enables the gall stones to pass either into the duodenum, or back again into the gall-bladder. If the impaction takes place in the cystic duct, pain without jaundice may arise; but if the obstruction occurs in the common duct, the patient becomes jaundiced, as the entire mass of bile—that secreted immediately by the liver, as well as that accumulated in the gall-bladder is prevented from passing off into the intestinal canal; and consequently, being absorbed into the general system, occasions a regular attack of jaundice. The latter was my case. In corroboration of these views, it is observable, that some animals, the horse for instance, are not furnished with gall-bladders; and the reason of this seems to be, that a reservoir for bile is only necessary for the assimilation of large quantities of food taken in at distant intervals. None of the carnivorous animals are without gall-bladders. They are capable of enduring long fasts, and then gorge their stomachs to excess; therefore a reservoir of bile for those frequent great demands upon the digestive powers of those animals is necessary. Herbivorous animals, with capacious stomachs, also re-

quire such a reservoir on account of the large quantity of nutriment they take in at a time, and are therefore furnished with gall-bladders. But the horse has a small stomach, capable of containing little food compared to that of other animals, consequently it must be the oftener replenished ; therefore there is no necessity in this animal for a reservoir of bile, in order to complete the digestion at any one time of a large quantity of chyme. The reason the horse is thus constituted is obvious. He is furnished with great locomotive powers ; but these powers would be useless, were he also supplied with a large stomach ; for when put to that speed which we so much admire in this noble animal, a capacious stomach filled with food would prevent the play of his diaphragm and oppress his lungs.

This view of the uses of the gall-bladder also serves to explain the origin of gall-stones. These bodies are nothing more than inspissated or impacted bile ; and such inspissation is most likely to occur in those who fast inordinately long, and in whom the demand for the bile in the gall-bladder occurs only at very distant intervals. The consequence is, that the more fluid or watery parts of the bile are absorbed, and that which remains is so thickened as to afford the nuclei of gall-stones. In support of this view, we should recollect that absorption is most active in the absence of due nourishment.

Hence, to look beyond myself to others, we perceive the reason why those who are most frequently the victims of this disease are generally the most intellectual part of mankind, as literary men, lawyers, physicians, and all such as in the ardour of pursuing their sedentary avocations, are unmindful of the necessity of supplying the system with due nourishment at regular periods. While it is equally observable, that manufacturers, artisans, shopkeepers, and all those who have their meals at regular and fixed periods, are comparatively little subject to this most distressing malady. If it were found, on inquiry, that savage nations, who live on the precarious supplies of the chase, and who therefore, after long fasts, gorge their

stomachs to excess, are prone to gall-stones, it would give the force of demonstration to those views, which, if correct, afford simple but important practical indications of the best mode of prevention, by enforcing the utility of taking our meals at regular and fixed periods, which I need not say ought to vary with the age, powers, and constitution of the individual. The younger the person, the more frequent the necessity of nourishment. The same may be said of invalids, who not possessing the powers of digesting a usual moderate meal, must supply the deficiency by repetition. But every sixth hour during the day may be esteemed the averaged time for the healthy adult, and certainly ought not to exceed every eighth hour.

From the preceding observations, let it not be imagined that I advocate the heavy meat luncheons so much in vogue. On the contrary, I am sure our profession is a hundred to one more indebted to the extremes of eating than to those of fasting, particularly amongst the better ranks of the softer sex, who in consequence of the enjoyment of a carriage almost forget how to use their legs except in a ball-room. Thus from their habit of over-exciting their stomachs with two dinners daily, of tight lacing, and of enjoying little or no exercise, there is no class of persons in society to whom the medical profession is so deeply indebted for patronage and support. Either let their usual lunch be their only dinner, or let them be satisfied with a light sandwich or a biscuit; or if they prefer it, let them continue, *Deo volente*, to force their delicate appetites with two dinners daily, sit tightly laced in their carriages, and take physic—and the medical world will thrive accordingly.

But to return to my own case. Notwithstanding the serious afflictions of dyspepsia and gall-stones, I continued to labour through my professional avocations during the summer of 1828 ; at the latter end of which, I first began to complain of pain in the hip, along the course of the sciatic nerve. This I thought to fight off by exercise ; but in vain ; the more I exercised, the more the pain increased, and ascending a flight of stairs be-

came soon far worse than the punishment of the treadmill. Still I laboured on, visiting my patients in a close carriage, but was reduced to the necessity of being carried up stairs to those who were unable to see me in the lower apartments. As an instance of my folly and perseverance, I recollect the observation of my friend Mr. Colles, while assisting me in two painful and tedious operations: "that although my patients seemed to suffer great pain, yet that he was certain that I suffered ten times greater."

After persevering obstinately in this unwise course for some months longer, I was at length obliged to give in, and confine myself to bed, in which I was not even able to turn without assistance. I now used a variety of remedies: mercury was taken so as to affect the gums; afterwards turpentine, until it occasioned stranguary; then the carbonate of iron in large doses; and lastly, the compound infusion of sarsaparilla in lime-water; while acupuncture, cupping, blistering, and tartar emetic ointment, were in succession applied over the course of the sciatic nerve and its branches. Of these remedies, mercury and turpentine seemed the most efficient; but nothing gave relief from acute pain, except large and repeated doses of opium; of the preparations of which, black drop was preferred, as it occasioned but little disturbance of stomach. When suffering under the tortures of this most severe neuralgic affection, a dose of black drop would, in fifteen or twenty minutes, not only relieve me from pain, but produce such a happy state of tranquillity and pleasurable sensations, that I had a narrow escape from becoming ever afterwards a regular opium-eater or drinker.

Sciatica appears to be nearly related to gout; for I was frequently awoke at night, with acute pain in the instep or ball of the great toe of the affected limb, so that I expected, on the return of morning, to see the usual inflammation of gout occupying these parts, particularly as I had known more than one instance of sciatica to be relieved by an attack of regular gout. However, this fortunate termination of sciatica did not fall to

my lot. Erratic pains through all the joints, unattended by inflammation, are by no means unusual in those who suffer under dyspepsia ; but in others, acute inflammation of the joints will succeed those pains ; and this is the disease to which, I believe, the popular term of gouty rheumatism, or rheumatic gout is applied—a good distinctive cognomen, indicating a species of rheumatism connected with derangement of the digestive organs, as contra-distinguished from that other form of rheumatism which arises from undue exposure to cold and moisture. This distinction I consider of great practical utility ; because in the first mentioned species we shall in vain endeavour to free our patient from his pains, until we improve the state of the digestive organs and general habit.

I am convinced that the great majority of neuralgic affections have their origin in the same causes, or state of constitution, as those erratic *gouty* pains (I make use of the term for want of a better) to which I have alluded. The affections of the fifth and seventh pairs of nerves, under the name of *tic douloureux*, and that of the sciatic nerve, under the term of *sciatica*, are those with which we are most familiarly acquainted. But there is not a part of the body whose nerves are not liable to this painful affection, which renders our diagnosis at times extremely difficult. The following circumstances would guide *my* judgment ; I should conclude the pains were neuralgic, if, though acute, they are fleeting, and not permanent. If they are unattended by pain on pressure, or by any of the local or constitutional signs of inflammation, or of hectic fever. If with these negative signs, we have positive indications of permanent disease in the suspected organ, we can seldom err in coming to a just conclusion.

Neuralgia of the breasts in females is a very frequent disease, and excites great alarm in the mind of the person affected, from the apprehension of cancer. The absence of induration or tumour, and both breasts being affected with the flying or lancinating pains, together with positive evidence of derangement of the chylopoetic viscera, or of the uterine system, point out the

true nature of the disease. However, in women of large breasts, there may be great difficulty in ascertaining the existence of tumour or induration, and in such persons we must give a very guarded prognostic, particularly if the pains occur between the fortieth and fiftieth years, the period of life most susceptible of cancer.

Neuralgia of the testes, extending to the perineum and pubes, is also not unfrequently met with. It most usually occurs in irritable persons, after a gonorrhœa virulenta, and excites great distress and apprehension in the mind of the patient. In several instances I have been successful in removing those pains by exciting a discharge from the urethra, by means of irritating bougies, or the application of nitrate of silver to the membranous portion of the urethra. My friend Doctor O'Beirne informs me, that he has been successful in removing neuralgic affections of the breasts and testes, by exciting a crop of pustules, with tartar emetic ointment over that part of the spinal cord which supplies these organs with nerves: a view well deserving of attention in our present state of uncertainty respecting the immediate causes of neuralgic affections; and, in corroboration of his views, I may mention, that I have found no means more effectual in removing the severe pains, particularly of the muscles of respiration, to which hysterical women are subject, than the irritation of tartar emetic ointment along the spine. I could adduce a considerable number of instances of the success of this plan, when assafoetida, valerian, ammonia, and all other medicines of this class had failed.

The sphincters of the anus and bladder are liable to be affected with neuralgia, which, independently of pain of a spasmodic character, produces most distressing symptoms arising from disturbance of the functions of muscles so important to the business of animal life.

Neuralgia of the knee-joint so closely resembles the symptoms of ulceration of the cartilages, that it is often exceedingly difficult to distinguish these diseases from each other, a circumstance which may lead to the most distressing errors in practice.

I have myself witnessed two instances of amputation performed by experienced surgeons, under the belief that their patients laboured under ulceration of the cartilages; and yet on examination of the amputated joints, not a vestige of organic disease could be discovered. I should imagine that this mistake is not unfrequent, as I have heard of other instances from a quarter upon which I can place reliance. If there is any doubt upon our minds respecting the nature of the disease, it would be wise to defer amputation until the symptoms of hectic fever make their appearance.

It is extraordinary to what an extent pain can exist in nerves, without any sensible alteration in their structure; but perhaps organic changes are induced, which our ignorance of the minute texture of nerves does not as yet permit us to appreciate. When the microscopic observations which are now going forward have discovered the difference between the minute structure (for I have no doubt but differences will be found) of the motive and sensitive nerves, and between the various portions of the nervous system which possess distinct functions, it will be then time enough to endeavour to ascertain the minute organic alterations which disease may induce. Many years since, in dissecting a subject, I found a portion of a needle, three quarters of an inch in length, imbedded in the very centre of the sciatic nerve, where it passes behind the great trochanter; and yet there was no thickening of the nerve or its investment, nor any of the usual signs of the effects of previous inflammation. Hence we may conclude, that the nervous structure is but little subject to inflammation and its consequences. But this admits of a doubt; for we often find that, after amputation, the most severe neuralgic pains occur in the stump, and in such instances the cut extremities of the nerves have been found indurated and enlarged. It is also said, that in those who have suffered severely from sciatica, the neurilemma of the sciatic nerve has been found thickened and enlarged. In one instance, that of a woman who died of tic douloureux in the Richmond Hospital about two years since, the Gasserian ganglion of the fifth pair of nerves was

found hardened and enlarged to the size of a nutmeg, and exhibited a fibro-cellular appearance. The nerves could not be traced through the tumour, but were lost in its substance as far as the tumour extended. I have the above account of the dissection from my colleague Dr. Hutton, who made the *post mortem* examination. This poor woman had been several years afflicted with the disease, from the tortures of which, neither opium in the largest doses, stramonium, nor any other narcotic afforded the slightest palliation; in fact she suffered more agony than *any* human being I ever saw afflicted with *any* disease. To so great a degree was she worn down by pain, that for some months before her death, she had almost the appearance of a living skeleton.

I think it is Dr. Wilson Philip who truly observes, that affections of parts, which were at first merely sympathetic, may ultimately become organic; (I quote from memory;) thus pain in the head, arising from a disordered stomach, if constant and severe, will at length induce that sub-inflamed state of the brain and its membranes, with consequent depositions, which constitute organic changes from the healthy structure. Now this being admitted, it follows, that any part sympathetically affected may ultimately become organically diseased, and this important view we should keep in our recollection, in endeavouring to trace effects to their causes, when we find organic changes in any portion of the nervous system.

Mr. Cæsar Hawkins, in his lecture on Tumours, (reported in Med. Gazette, March 10th, 1838,) gives a section to the consideration of tumours of nerves, which he admits, however, are not very common. He does not detail any instances from his own experience, but gives one from that of Sir Everard Home, of a tumour which was removed from the axillary plexus. It is described as having been situated in the texture of the nerve between its fibres, of a tolerably firm consistence, but with a mixture of fluid of a white or light brown colour.

Amongst the narcotic tribe of remedies, after opium, I have found stramonium the most useful in allaying the pains of neuralgia. In some severe instances of this affection of the nerves

of the face, the relief it afforded was instantaneous and satisfactory, but in others it failed altogether. The medium dose was a grain of the extract every sixth or eighth hour, which usually occasioned a sense of dryness in the throat, an indication of the effects of the medicine upon the system, and that it would not be safe afterwards to increase the quantity.

But it is time, after this long digression, to return to my own case. However much my pains were relieved by opium, my stomach and general health become so very much deranged, that it was obvious I could not long hold out under my then system of management, or rather of mismanagement. I therefore called a meeting of my medical friends, who all reprobated my folly in seeing patients while confined to my bed; they, therefore, highly approved of my proposal to leave the country, for the mild, warm climate of the south of France, and the waters of Barège. Indeed, I was myself convinced of the necessity of trying some new system, as my favourite resource, opium, was beginning to fail in affording relief. Two of my medical friends were, I understood, of opinion, that the malady under which I laboured was not sciatica, but disease of the hip joint. The distinctive signs, however, between the two diseases are so well marked, that I was, in my own mind, perfectly easy on this subject. The only symptoms in common between them is, the wasting of the glutei, as well as the muscles of the entire limb; but the two following circumstances may decide our judgment, should any doubt exist. First, when the heel of the affected limb is stricken, the impulse of the head of the femur against the acetabulum will excite severe pain, if the disease is in the joint; but none whatever, should it be in the nerve; and, secondly, in sciatica, the pain follows the course of the branches of the sciatic nerve, so distinctly, that in my own case I was most uncomfortably reminded of my former dissections of those nerves, particularly of the perineal nerve, where it passes near the head of the fibula. In the advanced stages of sciatica, the pains extend to the toes, and are accompanied by a most distressing sense of numbness of the foot. I need not say, that these symptoms do

not attend morbus coxarius, while a fixed pain in the knee is in the latter a well-known diagnostic system.

Rejoicing in the prospect of any new measure likely to afford relief, I was conveyed to the packet which sailed for Bordeaux on the 15th of June, 1829, where I arrived after a prosperous voyage of seventy-two hours, contrary to the expectation of many of my friends, who did not think that I should ever reach it alive. The night I arrived in Bordeaux afforded me a sound sleep, which I had not experienced during the previous year. The mild, warm climate of the South of France (so different from that of this country, which, although in the month of June, had been cold, wet, and variable) was delightful to my feelings, and from the effects of climate alone, I now anticipated a certain recovery. I remained a fortnight in Bordeaux, where I gained considerable strength. While there, the only medicine I took was La Fitte and Chateau Margau claret, strongly recommended to me by my friend, Doctor Millengen; and I believe these wines are the very best restorative tonics for broken down, debilitated frames, such as mine was at that time. I at first protested against claret, as I had found even a single glass of the best I could procure in Dublin to sour on my stomach. The intimation had no effect upon my inexorable medical adviser, who ordered me to take a pint of the wine he recommended the first day, and if I found it to agree with me to take a bottle every day afterwards, which commands I not unwillingly complied with; for to my great surprise I found this pure, unmixed wine agree remarkably well with me, and I have ever since taken special care to have my cellar well supplied with beverage of the same description. The fact is, that our habit of drinking strong wines has completely vitiated our palates. The claret, therefore, prepared at Bordeaux for the English market is rendered impure by a mixture of Hermitage and other ingredients, which give it a strong rich flavour; but which would be so distasteful to a French bon-vivant, that he would not allow it to touch his lips a second time. If medical men were better acquainted than they are, with the supe-

rior qualities of pure clarets of *the first growth*, which is the term, I believe, used by traders in wine, they would not be so much in the habit of recommending strong Sherry upon all occasions where the use of wine is indicated.

I now proceeded, by easy stages, to Bagnères de Bigorre, which is situated at the French side of the central Pyrenees, in the beautiful valley of Campan : it contains between twenty and thirty springs, of which, by the advice of Doctor Ganderax, Inspector of the Mineral Waters, I drank only of the Lascere spring ; of this I took two large glasses (equal to a pint) on going into a warm bath, at the temperature of 96 of Farenheit, and after remaining in the bath a full hour, repeated the same dose ; so that I took each morning, before breakfast, a quart of this warm mineral spring. The Lascere waters contain various saline ingredients, in such minute quantities, that they are scarcely perceptible to the taste, and, according to the analysis contained in the work of Doctor Ganderax, is as follows :—

In twenty-five kilogrammes (a kilogramme is exactly 2lbs. 8oz. 1 dr. 24 gr., that is, about equal to our imperial quart), there were found the following ingredients :

The quantity of carbonic acid was inappreciable.

	gr.	c.
Hydro-chlorate of Soda,	1	15
————— Magnesia,	4	30
Sulphate of Lime,	45	80
————— Magnesia,	10	20
Sub-carbonate of Lime,	5	75
————— Magnesia,	1	55
————— Iron,	0	45
Resinous substance,	0	10
Vegetable extractive do.,	0	18
Silica,	1	0
Loss,	0	52
Total,	71	0

Notwithstanding the small proportion of saline ingredients, (seventy-one grains in twenty-five imperial quarts of water), and none of them of a very active nature, I derived decided, and almost immediate, benefit from their use. The appetite improved, the tongue became clean ; there was no longer a painful sense of distention, even after a slight repast. The bowels became regular ; and the urine, which had been constantly turbid, and loaded with a deep lateritious sediment, became clear, of a healthy amber colour, and considerably increased in quantity. The neuralgic pains now seldom required the tranquilizing influence of opium, and I had the pleasure of enjoying a sound sleep every night. We, therefore, must attribute the virtues of these waters not only to their temperature, but to their unknown state of combination. In no instance are we authorized to estimate the utility of mineral waters by the quantity or quality of their saline ingredients, of which the Lascere spring of Bagnères is a sufficient proof.

I now ventured to move about on crutches, with the foot of the affected side supported in a sling, so that the limb remained perfectly passive. And here I shall take the opportunity of observing, that in the treatment of sciatica, it is of the utmost importance to prevent the patient from making use of the affected limb ; for, during the acute stage of the disease, any muscular exertion of it is soon after followed by severe pain ; and even during the chronic stage, when the patient fancies himself convalescent, and almost well, it is sure to prevent that favourable result : so that nothing requires more consideration than the time at which the patient may be permitted to enjoy the use of the limb ; he will be, perhaps, the best judge himself, giving him warning of the ill effects of premature exertion. The *passive* exercise of a carriage, or that on crutches in the way mentioned, the foot in a sling, is not only agreeable, but, I believe, useful, when the disease has arrived at its chronic state.

The great advantage I derived so quickly from the use of the Bagnères' waters has since induced me to order, in dys-

peptic cases, *attended with turbid urine*, a small proportion of neutral salts, dissolved in a large quantity of warm water, to be taken every morning, an hour or two before breakfast ; during which time I enjoin some exercise either on foot or horseback. The ingredients of my saline powders are, from twenty to thirty grains of the bi-carbonate of soda, to sixty or ninety grains of Rochelle salts—the tartrate of soda and potash ; to this, if a chalybeate is indicated, a couple of grains of the sulphate of iron is added. This I direct my patient to take in half a pint, or even a pint, of warm water, (temp. 90 to 94), if his stomach can bear it, which it usually can, if divided into two doses, allowing an interval of half an hour between each. Now this simple medicine, with three or four grains of blue pill every night, or every second night, according to the state of the biliary secretion, with due attention to diet, I have found of more advantage in dyspeptic cases, attended with turbid urine and lateritious sediments, than all the farrago of tonic and bitter medicines usually resorted to. In some few instances, when there appeared to be an uncommonly irritable state of the mucous membrane of the intestinal canal, neither this nor any other saline medicine, no matter how minute in quantity, would be advisable. But in the great majority of cases of dyspepsia, attended with turbid urine, I have been so much in the habit of prescribing these powders, from ample experience of their utility, that the apothecaries have christened them “ *Car-michael’s powders* ;” and I have reason to know that some of these gentlemen laugh at my powders, as silly and inefficacious, of no use whatsoever to the patient, and but very little to the apothecary. However, I find that some practitioners, having ascertained their value, have prescribed them under the form of nice little ounce draughts, prettily coloured, by the addition of compound tincture of cardamom ; so that under this new appearance, I was scarcely able to recognize my old friends. But if these discreet practitioners do not direct their patients to dilute their pretty pink-coloured draughts with a large quantity

of warm water, they will not be likely to derive credit to themselves, or confer benefit on their patients.

Much advantage is no doubt derived in dyspeptic cases from the use of the warm bath, and the long period (seldom less than half an hour, but in general a full hour) in which patients are accustomed to use it on the Continent; for it always improves the functions of the skin, and on this account I am much in the habit of prescribing the tepid salt water bath for the treatment of dyspepsia; a dry skin being so very frequently an accompaniment of a deranged state of the chylopoietic viscera. Or I direct the patient to sponge his body every morning with tepid water, almost saturated with common salt; and when he has dried himself, to make use of powerful, coarse flesh-brushes, until he excites the action of the capillary vessels of the skin even to redness. Exercise in this way is doubly beneficial, for it not only exercises the muscles of the patient, but that exercise is expended on himself in exciting to healthy action the capillaries of the skin, and in removing the accumulated scalliness of the cuticle. By these means the secretion of perspirable matter is promoted, which, together with the urine, eliminates from the system its various exhausted and acidified materials. It is worthy of observation, that both of these secretions, in a healthy state, betray the presence of a free acid. The secretions deposited in the small and large intestines on the contrary, in a healthy state, show no signs of acidity; but in diseases where the skin is dry and parched, and the urine is pale and abundant, denoting alkaline instead of acid properties, the secretions into the small intestines are found to be acid, and even the bile in the gall-bladder, which in its natural state contains a free alkali, may become under these circumstances acidified. But in the various examinations I have made, I never found the presence of acid denoted in the large intestines, although the contents of the small intestines in the same subject changed every where vegetable blues to a red. These circumstances, which I ascertained long since, and published in

my Essay on Scrofula, indicate in the strongest manner the necessity of attending to the state of the secretions of the skin and kidneys in derangement of the chylopoietic viscera. Chemists have not yet determined on the precise nature of the free acid contained in these secretions; whether it is the uric, the lactic, the phosphoric, the muriatic, or the sulphuric. It is in all probability the uric, as it is the weakest of those acids, and therefore most liable to be separated, or disengaged by the superior affinity of the other acids for the alkaline bases which enter into the composition of these secretions.

From the observations and experiments of Doctor Beaumont of New York, on the young man already alluded to, who received a large gaping wound in his stomach,* we learn that the gastric juice is slightly acid, and only secreted when required, that is, on the admission of food into the stomach. He also informs us, that it is strongly acid in herbivorous animals, while in carnivorous it does not betray the slightest trace of acidity. Hence we perceive the reason why we take vinegar with salad, salmon, and all food of difficult digestion; as the acid thus taken in assists the acidified and solvent powers of the gastric juice; and hence we may learn that the indiscriminate use of alkalies and absorbent earths has been attended with much disadvantage to those who suffer under acidity of stomach, in whom it never affords more than temporary relief. While mi-

* This wound was admirably calculated to admit of observation and experiments respecting the process of digestion; for a small fold of the villous coat of the stomach acted as a *valve*, so as completely to prevent any afflux from within, but to admit of being easily pushed back by the finger from without. Dr. Beaumont made the best use possible of the opportunity he thus possessed, of observing the phenomena of digestion in a series of observations continued during eight years, which afford more certain information on this interesting subject, than all the works that ever were written. I never could procure the original essay, but read with great satisfaction an account of Dr. Beaumont's observations and experiments in Dr. Andrew Combe's admirable work on "The Physiology of Digestion considered with relation to the Principles of Dietetics."

neral acids, particularly the nitro-muriatic, on the contrary, are very effectual, according to my experience, in curing permanently this troublesome symptom of dyspepsia. From Doctor Beaumont's observations on this same individual, we also learn that no gastric juice is secreted during the existence of fever; hence the injurious consequences of taking solid food into the stomach when it cannot be digested, and therefore must only oppress that organ, until rejected by vomiting. Drink is the only diet fitted for fever patients, which is imbibed by the absorbents and veins, without undergoing any process of digestion.

But to return from this long digression to my own case. My improvement was so rapid, that in a fortnight after my arrival at Bagnères, I was enabled to get on horseback, and thus I had the means of enjoying the delightful climate and magnificent scenery of the Pyrennees. After a month's residence at Bagnères de Bigorre, I ascended the mountains to Barège, which is estimated at 4190 feet, while Bagnères is but 1219 feet above the level of the sea. The climate of Barège is consequently so much colder, that few resort thither before the middle of June, and it is nearly deserted by the beginning of October. This village is situated in a deep valley or cleft of the mountains, which are formed chiefly of gneiss and micaceous and argillaceous schist, appearing in many places like green or blue roofing slate. It is only inhabited during the summer months, for the houses are shut up during the winter and spring. The numbers that flock to this remote and extraordinary village from all parts of Europe, render it extremely difficult to obtain accommodation, and lodging is consequently very expensive. The waters are sulphureous, and of a very high temperature, from 30° to 45° of Reaumer, i. e. from 100 to 135 of Farenheit. They are extremely nauseous to the taste, and exhale an odour like rotten eggs, so that few stomachs can bear them. It is therefore to their external use that they owe all their celebrity. I have no exact analysis to lay before you; but according to Alibert in his work on Mineral Waters, they contain hydro-

chlorate of magnesia, chlorure of sodium, sulphate of magnesia, sulphate of lime, carbonate of lime, sulphur, silica, and an oleaginous or saponaceous substance, which chemists have in vain endeavoured to imitate. He informs us that Mr. Longchamp has ascertained that they contain soda in its uncombined or caustic state, and that the saponaceous matter mentioned above is an animal substance which he calls *Barregine*, and is essentially different from *Gelatine*, which is used in the manufacture of factitious Barége waters.

I was obliged to wait a fortnight, so numerous were the claimants, before the inspector of the mineral waters could grant me leave for the douche, although half an hour is only given to each individual, and a succession of patients, during both night and day, anxiously wait for their turn to partake of this fountain of health. In the interval I, however, used the warm bath, at the temperature of 96°, from which I received decided benefit. My turn at length arrived for the douche, which I had the option of taking either mild or severe: I preferred the latter. Eleven o'clock each night was the time appointed, and I considered myself fortunate to be permitted to use it even at this inconvenient time. At the appointed hour every night, I found a chaise-a-porteur at my lodging; in this I was conveyed in a few minutes to the douche. By the light of a glimmering lamp I found myself in a cell or dungeon which appeared to be cut out of the rock; it was, however, so hot with sulphureous vapour, that at first I felt nearly suffocated, and I was glad to disencumber myself as quickly as possible of my clothes, in which I was assisted by a surly, grim old attendant, who seemed naturally to appertain to a place filled with fire and brimstone. As soon as I was stretched upon a mattress which lay upon the floor, he turned a large cock about ten feet from the ground; the water, which was at the temperature of 120° Farenheit, fell therefore with considerable force, and such was the shock which I at first felt, that I could scarcely refrain from crying out; however, I summoned resolution, and bore it for fifteen minutes, which my at-

tendant said was the longest period that any person had been able to suffer under this infliction, ten minutes being the usual time. By shifting my position, I was enabled to let the water fall in succession over the entire of the trunk of the sciatic nerve and those branches which had been most painful. I found it very fatiguing as well as warm work, and that I really required the assistance of my grim attendant to put on the flannel dresses with which I had come provided. This being done, I was immediately reconveyed by the same machine into my bed-room, where, without taking off my flannel dress, I got into bed between the blankets, and in a few minutes afterwards was covered with a profuse perspiration, which continued for four or five hours, and was promoted by drinking freely of some mild warm beverage. As soon as it had ceased, I changed my flannel dress for another, and remained in bed for several hours afterwards.

I have been thus minute in my detail of trifling particulars, for the benefit of those who purpose going to Barége. The difficulty even of obtaining lodging is so great, that it would not be advisable for any patient to go there until he had secured one, and also permission from the inspector of the mineral waters to have a time allotted for his douche or bath ; and he should also take especial care to go well provided with changes of flannel dresses. I continued the use of the douche daily, or rather nightly, for a fortnight ; during which time I suffered under an increase of the pains, thirst, frequency of pulse, and other signs of slight fever. This is the usual effect of these waters when first employed ; and indeed I am not surprised that they should occasion great constitutional disturbance, when we consider the excitement occasioned by the douche, not only upon the cutaneous capillaries of the parts upon which it falls, but upon the entire surface of the body, which literally becomes of a red heat under its influence. Discipline of this kind for fourteen successive nights would pull down the strongest person ; and I, of course, found myself at the end of that time considerably

reduced in flesh. It is deemed a very unfavourable sign, when the douche does not excite perspiration, and in such instances, I am certain, much mischief must follow its use. From this account of the effects of the Barége waters, it is obvious that it is a most powerful instrument, capable of effecting much good or much evil. If inflammatory action is present, I conceive it must do mischief, and I am certain that I could not have borne the douche three months before I used it. If there is any inflammatory affection of the head or chest, the use of the douche must be attended with the utmost danger, and more than one instance of apoplexy and hemoptysis occurred during the short period of my visit to Barége. Great numbers of paralytic patients frequent this place. But as we have reason to suspect in all such cases some latent head affection, the douche should be resorted to with great caution. In no such instance ought the strong douche, however, to be employed, the weaker one must be comparatively more safe.

Partial paralysis, depending upon some affection of the spinal cord; various neuralgic affections; chronic lumbago, and sciatica; chronic rheumatism and gouty affections of the joints; various cutaneous diseases, such as lepra and psoriasis, *et hoc genus omne*, are the complaints which have a fair chance of receiving benefit from these powerful waters. Their great utility in old, exfoliating, gun-shot wounds has been so well established, that it is many years since the French government founded a military hospital, into which all appropriate cases are sent for three successive summers. But if the patient after this trial is not found fit for duty, he is either pensioned or discharged the service. I was informed that old wounds submitted to the influence of the Barége baths, enlarge at first under their use; and that even many of those that have been healed, will again ulcerate; this is no doubt owing to the excitement of the absorbing system, and the consequent destruction by ulceration of bad granulations, or the weak cicatrices of such as have imperfectly healed; but that afterwards most satisfactory cures take

place in old wounds which had resisted every other mode of treatment. The military patients are to be seen at all times in a large bath capable of holding twenty persons; they are marched down in successive divisions, each division remaining in the bath a full hour. Amongst those who recovered from grievous gun-shot wounds, I should not omit to mention our countryman General Crawford, who after passing three successive summers here, was established in perfect health. Grateful for the benefit derived from these waters, he gave, during his life, twelve hundred francs annually, for the benefit of the poor who frequent them; which donation has ever since been continued by his heirs.

The waters of Barége are but little used internally. They are said to be stimulant and diuretic, but are not in the slightest degree aperient. They disagree, however, so much with the generality of stomachs, that few persons can be prevailed on to persevere in their use.

The lightness of the atmosphere must no doubt have a considerable influence in the removal of many chronic diseases, for, as I before remarked, Barége lies in a valley 4190 feet above the level of the sea. Its great elevation will be more perfectly comprehended by my hearers, when it is understood, that this is an elevation six hundred feet higher than the top of Snowdon in North Wales. The effect of the attenuated atmosphere of so elevated a region upon diseases, may be in some degree appreciated, since I have been assured by resident medical men, that when the country people in the plains are affected with ague, they ascend the mountains as high as Barége, and without any other means become rapidly well of their disorder.

It is to be regretted that the French government does not increase the number of baths and douches, for there is, I understand, an inexhaustible supply of water; but there is not satisfactory accommodation of any description for one-half of the numbers who frequent this place.

After using the douche another fortnight, the change in the weather to cold, some showers of snow, and heavy torrents of

rain, gave us notice that it was time to depart. It was now approaching the latter end of September, and the floods from the mountains began to make such ruts in the only road which leads to this village, as to render it difficult to pass. We were informed that very soon it would be totally destroyed by those torrents—that the stupendous mountains, which hang over Barége, would be whitened with snow, and avalanches threaten its destruction. There is one part of the village most exposed to avalanches, which is only occupied with temporary wooden houses, erected at the beginning and taken down at the end of the season. Barége consequently becomes deserted by all human beings during the winter, and in their place, I was told, is tenanted by wolves and bears from the mountains. I therefore bid adieu to a place, which I shall always recollect with feelings of pleasure and gratitude, for I was completely free from pain, and never since had any return of the symptoms of sciatica.

After leaving Barége I took a delightful tour on horseback through perhaps the most wild and beautiful scenery in the world ; for the valleys of the Pyrenées may well compete with those of Italy or Switzerland. The view from the Pic de Bergon in the neighbourhood of Barége, from which you behold the grand boundary chain of mountains that separate immediately France from Spain, the circle of Gaverney, and the celebrated Brèche d'Orland, is unique, and perhaps one of the finest in the world. I visited many of the mineral springs with which the central Pyrennees abound, which, with the exception of Barége, are situated in the most beautiful valleys of these mountains. Saint Saver, Cauteret, the Eaux Bonnes, Bagnères de Luchon, and Bagnères de Bigorre, already noticed, are the most celebrated. These waters are all of a high temperature, and much frequented by visitors from every part of France during the summer season ; but have comparatively received little attention from the hordes of English, which annually sweep over the other parts of the Continent. However, when the beneficial influence of

these waters and the beauty of the valleys of the Pyrenées are more generally known, they will perhaps flock in as great numbers to them, as they do at present to the fashionable watering places of Nassau ; particularly as great facilities are afforded by the Irish Steam Packet Company for this excursion, by the fine steam vessel which leaves Dublin twice a month for Bordeaux during the summer season, and which on its way touches at Plymouth for the accommodation of English passengers.

Independently of any practical hints which the recital of my case may inculcate, it certainly affords a good moral lesson to the members of my own profession, indicating the folly of not *giving in* at once, when assailed by disease. The spasms and gastric fever, from which I narrowly escaped with life, were occasioned by over-exertion, long fasting, and a total disregard to the organic laws. My return to professional business long before my strength was restored after this fever, and while I was scarcely able to drag one leg after the other, occasioned my subsequent ill health and all the tortures of sciatica and gall-stones. Even when unable to move without the greatest pain from the first of these maladies, I still continued, in despite of warnings the most urgent, to engage actively in the turmoils of practice. The fact is, that of all individuals when suffering under disease, the medical man has the least chance of recovery. The public who place confidence in his skill will take no excuse ; and the more difficult he is of access, the more urgent will be their demands for him. His only safety is in flight ; and had I been as wise at the first approach of illness, as from woeful experience I am at present, I should have escaped the infliction of those painful and protracted complaints. I shall conclude by strongly recommending you to pay more attention than you are likely to do to the wise old adage quoted by Lily, "*Felix quem faciunt aliena pericula cautum.*"

BIBLIOGRAPHIC NOTICES.

SIR DAVID BREWSTER'S *Theory of Cataract*.

IN the number of the London and Edinburgh Philosophical Magazine for January last, is a paper by Sir David Brewster, "On a Singular Development of Polarizing Structure in the Crystalline Lens after Death; and on the Cause, the Prevention, and the Cure of Cataract." With regard to the first of these heads, viz., the Change in the Polarizing Structure of the Lens, it would be foreign to our present purpose to offer any remarks; before saying any thing on the subject of his opinions on Cataract, it may be well to put our readers in possession of them by a few extracts—

"Having submitted to the physical section, an account of a singular change of structure, produced by the action of distilled water upon the crystalline lens after death, Sir David Brewster was desirous of communicating to the medical section, some views, which this and previous observations, have led him to entertain, respecting the cause, the prevention, and cure of cataract.

"The change of structure to which I have referred, consists in the development of a negative polarizing band or ring between the two positive rings nearest the centre of the lens; the gradual encroachment of this new structure upon the original polarizing structure of the lens; and the final bursting of the lens, after it had swelled to almost a globular form by the absorption of distilled water.

"As the crystalline lens floats in its capsule, there can be no doubt, that it is nourished by the absorption of the water and albumen of the aqueous humour, and that its healthy condition must depend on the relative proportion of these ingredients. When the water is in excess, the lens will grow soft, and may even burst by its over-absorption; and when the supply is too scanty, the lens will, as it were, dry and indurate, the fibres and laminæ, formerly in optical contact, will separate, and the light being reflected at their surfaces, the lens will necessarily exhibit that white opacity, which constitutes the common cataract.

"This defect in the healthy secretion of the aqueous humour, as well as the disposition of the lens to soften or to indurate by the excess

or defect of water, may occur at any period of life, and may arise from the general state of health of the patient; but it is most likely to occur between the ages of forty or sixty, when the lens is known to experience that change in its condition, which requires the use of spectacles. At this period the eye requires to be carefully watched, and to be used with great caution; and if any symptoms appear of a separation of the fibres or laminae, those means should be adopted, which, by improving the general health, are most likely to restore the aqueous humour to its usual state.

“In so far as I know, cataract in its early stages, when it may be stopped or cured, has never been studied by medical men; and, even when it is discovered, and exhibits itself in white opacity, the oculist does not attempt to reunite the separating fibres, but waits with patience till the lens is ready to be couched or extracted.

“Considering cataract, therefore, as a disease, which arises from the unhealthy secretion of the aqueous humour, I have no hesitation in saying, that it may be resisted in its early stages, and in proof of this I may adduce the case of my own eye, in which the disease had made considerable progress.

“One evening I happened to fix my eye on a very bright light, and was surprised to see round the flame a series of brightly-coloured prismatic images, arranged symmetrically, and in reference to the septa to which the fibres of the lens are related. This phenomenon alarmed me greatly, as I had observed the very same images in looking through the lenses of animals, partially indurated, and in which the fibres had begun to separate. These images became more distinct from day to day, and lines of white light, of an irregular triangular form, afterwards made their appearance. By stopping out the bad parts of the lens by interposing a small opaque body, sufficient to prevent the light from falling upon it, the vision became perfect, and by placing an aperture of the same size in the same position, so as to make the light fall only on the diseased part of the lens, the vision entirely failed.

“Being now quite aware of the nature and locality of the disease, though no opacity had taken place so as to appear externally, I paid the greatest attention to diet and regimen, and abstained from reading at night, and all exposure of the eyes to fatigue and strong light. These precautions did not at first produce any decided change in the optical appearances occasioned by the disease; but in about eight months from its commencement, I saw the coloured images and luminous streaks disappear in a moment, indicating in the most unequivocal manner, that the vacant space between the fibres or laminae had been filled up with a fluid substance transmitted through the capsule from the aqueous humour. These changes took place at that period of life when the eye undergoes that change which requires the use of glasses, and I have no doubt that the incipient separation of the laminae would have terminated in confirmed cataract had it not been observed in time, and its progress arrested by the means already mentioned. Since that time, the eye, though exposed to the hardest

work, has preserved its strength and is now as serviceable as ever it had been."

Against the assertion "as the crystalline lens floats in its capsule," we must enter our protest; the best anatomists have given up the notion of the existence of a fluid, the liquor Morgagni, between the lens and its capsule, except in some cases of disease. We can mention no higher authority than that of Dr. Jacob, who says; "I do not believe that any such fluid exists in a natural state."*

It is also quite a gratuitous assumption to say, that the lens is nourished by the absorption of the water and albumen of the aqueous humour; this would be contrary to the analogy of the whole human frame. The lens obeys the same laws as the other parts of the body, and its particles are deposited and absorbed by vessels of its own, and the change which takes place in consequence of old age, namely its shrinking and flattening, and consequent diminution of refractive power, is not from defect of aqueous humour, but is an alteration which takes place in every part of the body; the nerves, with advancing age, become less susceptible of impressions; and the arteries, the distributors of nourishment to every structure, become themselves the subjects of calcareous deposits and other changes, by which their function is greatly impaired, the blood circulates imperfectly, and every organ feels the diminished supply, the eye in common with the rest; besides the change in the lens, the cornea becomes flattened, and its transparent extent encroached on by the opaque ring of the *arcus senilis*; the vitreous humour does not escape, and there is another part, without the integrity of which the utmost perfection in the mechanical arrangement of the rest of the eye would be useless, viz. the retina, whose sensibility becomes gradually but certainly diminished. When the lens is broken up with the cataract needle, and fragments of it are pushed into the anterior chamber and float about in the aqueous humour, do they become larger? are they nourished by the aqueous humour? no, absorption of their particles goes on, but being torn from their vascular connexions no reproduction takes place, and they soon disappear. But there is a stronger case. By violence the capsule of the lens is sometimes broken, and the aqueous humour, through the wound, comes in contact with the lens, which is speedily absorbed. Now contrast these instances with the fact of the dead lens immersed in distilled water, becoming daily larger and larger, as it was soaked in the fluid, and it must strike the most unthinking, that dead matter is subject to influences which

* Cyclop. Anat. and Physiol. pp. 200.

living matter resists. When the abdomen of a body some time dead is opened, the first thing that usually strikes us is the yellow tinge which all the organs in the neighbourhood of the gall-bladder receive from the bile. This we know is not so during life, the living gall-bladder retains its fluid contents perfectly, but as soon as death takes place the bile begins to ooze through its walls. It would be needless to push the argument further; enough has been said to show the caution that should be used in the application of phenomena observed in dead matter to explain the actions of the living body, and that Sir David Brewster's opinion, not only being merely founded on the agency of substances on dead matter, but being contradicted by living phenomena, falls to the ground.

As to his own case, which he brings forward in support of his views, it is obviously one of functional amaurosis, from sympathy with deranged stomach, or system generally, together with over-exertion of the organ, and which the treatment he adopted, regimen, diet, and rest of the eyes, was well calculated to remove. The appearance which alarmed him so much, the circle of brightly coloured prismatic images round the candle, is not an uncommon symptom of derangement of the functions of the retina, its sudden appearance, and its disappearance in a moment, are quite characteristic of the nervous nature of the affection, as well as the fact of his being able to cover the bad parts, by placing before them an opaque body; functional amaurosis being frequently only partial, as in the instances of a person in reading seeing only some of the letters, half a page, and the like. The great oversight of Sir D. Brewster is forgetting the retina, the nervous apparatus of the eye, whereby alone we are sensible of the existence of light, and of the image impinged on the back of the retina, and merely looking on the eye as a mechanical optical arrangement; he should never have forgotten, that no matter how perfect this mechanical arrangement might be, if the function of the retina is disturbed, the strangest aberrations of vision occur. Even where the optical mechanism has been considerably altered, the retina has often exhibited the most wonderful powers of adaptation, as in some cases where the lens has been removed by operation, and after a time the persons have had vision sufficiently good to enable them to dispense with the use of glasses.

Perhaps there is no symptom which so well exemplifies the difficulty of explaining derangements of sight, as that of double vision. I shall mention a few instances.

In the last number of the *Dublin Journal*, is related by Dr. Robertson, a case of aneurism of the carotid artery. This so affected the circulation in the head, that double vision resulted,

so complete, that the gentleman had to give up hunting, as he, saw two gates, &c.

I attended, with Mr. Crampton, a young gentleman with confirmed disease of the brain; the most remarkable symptom, and which first led him to seek advice, was double vision; one object was seen as the real one, the other as the shadow; the object became single on his closing the eye. The next change was, that the object and shadow became not only more separate and distinct, but crooked.

In these two cases, the derangement was in the brain—in the first, of its circulation; in the last, in its substance: the function of the retina became in consequence disordered. In the next case, the same result took place from sympathy, with a distant organ, the stomach.

A gentleman ate a small piece of pastry between meals, and had immediately uneasy feelings about the stomach. On walking in the open air, his sight became dim, and he had double vision, accompanied with some headach, all which went off the following day.*

A man fell from a scaffold forty feet high; he came on his feet, but then fell violently on his shoulder and side of the head, and bruised the left eyebrow. When admitted eight weeks after, under Mr. Crampton's care, it was found, that he could scarcely raise the left eyelid at all; when it was raised for him, and he was desired to look up, it was evident that he could not turn up the eyeball on the affected side to an equal degree with that of the other side. Vision was double; if either eye was closed it became single.

A woman came to me, with a tumor under the edge of the orbit, which pressed the eye down below the level of the other eye; when she looked down, she had double vision; it ceased when either eye was shut. The increase of the tumour and further displacement of the eye, finally completely extinguished sight, but the tumour subsiding under treatment, sight gradually returned, and became again double.†

In these cases the mechanical alteration of the position was the cause, and the explanation is obvious, that the images falling on non-corresponding parts of the retinas, conveyed a double impression to the brain; and this effect, at least, we should expect to be constant: but to show how really obscure the explanation of optical phenomena is, I have at present under my care a woman, who has a precisely similar tumour under the edge of the orbit, displacing the eye downwards considerably below the level of the other eye, yet there is no double vision.

* See Wardrop on the Morbid Anatomy of the Eye.

† Dublin Medical Journal.

The effect on vision is exactly what is produced by pressure with the finger on the globe of the eye. First, objects became faint, and as if covered with a gauze, and finally extinct, producing total blindness of that eye; the mechanical cause, however, *still remaining*, this state did not continue, but vision slowly returned, and though the tumour is less, the eye is still so much displaced, that one might look for some disturbance of sight, but there is none. In the first case, on the other hand, two years after, when all trace of the tumour had gone, and the eye in every respect appeared natural, the patient had double vision when she looked down. I have ventured to bring forward these instances, because they appear to set in a strong point of view, how the presence of a vital principle and nervous system, which should never be overlooked, as they have been by Sir David Brewster in this instance, modify the actions of organs. Derangement of the circulation of the brain, disease of its substance, altered action of the muscles of the eye, and tumour causing mechanical displacement, sympathy with a distant organ—the stomach, all producing the same effect, and even that cause, viz. the mechanical one from which we might look for the most uniform result, from some power of adaptation in the retina, disappointing our expectation.

Nothing could show better how a fixed idea in the mind, silences for a time, the more sober dictates of common sense, than the following extract.

“ If the cataract had made greater progress, and resisted the simple treatment which was employed, I should not have hesitated to puncture the cornea, in the expectation of changing the condition of the aqueous humour by its evacuation, or even of injecting distilled water or an albuminous solution into the aqueous cavity.”

The mere letting out the aqueous humour might possibly have been unattended with any great harm, but from the second plan it is reasonable to suppose, that the wound of the cornea, its irritation by the pipe of the syringe, the forcible introduction of a strange fluid among the delicate textures of the eye, would have produced such an inflammation, as would have cost Sir David Brewster that precious organ,—a dear price, indeed, to pay for a theory.

J. H.

Practical Observations on the Pathology and Treatment of Typhus Fever. By ALBERT HENRY CALLANAN, M. D.

DR. CALLANAN published the first edition of this work in 1817, and consequently the present edition is enriched by the prac-

tical experience of twenty years. He still retains Armstrong's classification, as well adapted to mark the chief varieties of this formidable disease. They are termed the *simple*, the *inflammatory*, the *sub-acute inflammatory*, and the *congestive* typhus. As we are of opinion, that there is no subject upon which the views of practical men ought to be more carefully recorded, than that of fever, we have subjoined, without abridgment, the whole of what Dr. Callanan says on the subject of treatment.

“ On the accession of typhus fever, the use of emetics has been loudly extolled, and I am disposed to concur in the praises bestowed upon them. They, however, will disappoint those who may be disposed to confide too much in them. Under the appearance of nausea, or that deadly sickness of the stomach, which sometimes accompanies the first attack of this complaint, much good will probably result from their use. In these cases, it will generally be found, that the biliary organs are in a state of increased activity, and hence that great accumulations of bile will take place in the duodenum and the proper receptacles of this secretion. I lately witnessed two instances of this kind, where, at the accession of typhus fever, spasm of the duodenum, caused by the irritation of acrid bile, continued for two days, and produced complete obstruction of that bowel. The quantity of bile discharged by the stomach during this time would be almost incredible were I to specify it. In both these cases the skin was hot and dry, the pulse full, and the region of the liver sore and tense. Venesection was had recourse to, the obstruction was speedily removed, the secretion of bile was effectually diminished, and mild typhus fever followed in both instances, which continued in one to the fifteenth, and in the other to the seventeenth day. Again, this state of nausea and vomiting will frequently usher in one of the most dangerous forms of typhus fever, namely, that in which the brain and its investing membrane are the points to which the destructive influence of the disease is directed. In these cases the patients are frequently walking about, and say they are *bilious*; but in combating a vision, that time is frequently lost which can never be recalled, and consequently those means of relief are neglected, the application of which will now be utterly unavailing. It will also happen, that where the cold stage of typhus shows a disposition to continue for a longer time than natural, an emetic, by tending to restore the circulation, and thus to relieve the pulmonary and hepatic systems, and indeed all the internal organs, from what have been appositely termed *engorgements* of blood, will frequently be productive of incalculable benefit. Some physicians, and amongst them Doctor Armstrong, appear to think their use either precarious, or inadmissible. Taking into consideration the principle of action just now assigned to them, I cannot admit the force of any objections I have heard urged against their probable efficacy. It must be observed, however, that when they combine, as they frequently do, with their immediate and proper

operation, a purgative effect upon the bowels, the benefit resulting from their use will be more likely to be effectual and permanent, than where their operation is limited to the stomach alone. With regard to the use of particular medicines of this class, I am disposed to prefer a combination of tartrate of antimony and ipecacuanha, in such doses, and repeated at such intervals, as will secure a full effect within two or three hours from their exhibition. Ten grains of the latter, and half a grain of the former, given every half hour, until full vomiting shall take place, have, in most instances in which I have used them in these proportions, secured the effects I wished for. Tartrate of antimony alone, given in divided doses, will frequently effect this purpose, but I do not think it can be so implicitly relied upon as in the combination I have mentioned. Full doses of purgative medicines succeeding this operation will very frequently arrest the progress of even the inflammatory species of typhus. Calomel, either given alone, and followed up by repeated doses of neutral salts or castor oil, or combined with rhubarb, will generally answer this indication. To render their effect certain or decisive, however, it will be necessary to have recourse to these measures within *the first three days* from the commencement of the disease.

“ If after these measures shall have been carried into effect, the symptoms of excitement should continue unsubdued, we have reasonable grounds for apprehending the existence of something more than the presence of the milder form of typhus; and here we come to the question whether other and more active means of depletion may be safe or necessary. In the majority of cases of this nature, it appears to me, that the pulse and the temperature of the body will be the surest guides for us to follow. The respiration too, particularly after the cold stage has gone by, will tend to throw much light on the real nature of the complaint. Where it is ascertained that *reaction* is fully established, and the breathing continues hurried, anxious, and labouring, we may be certain that in the development of this process, irregularity and undue distribution in the circulation of the blood have fallen upon some of the great organs of life, and are interfering with their functions, and thus laying the foundation of future mischief.

“ Under such a combination of circumstances, it is obvious that something more than mere palliative treatment will be necessary. To strike at the root of evil, therefore, it will be requisite not only that we relieve the organs immediately affected, but also that we should adopt such measures of cure as will reduce the frequency and force of the general momentum of the blood, and thus prevent the future tendency to that inequality in its distribution, which constitutes the peculiar modifications of typhus fever under consideration.

“ To obtain this end, I am unreservedly of opinion, that nothing will give us so fair or so rational a prospect of success as depletion either general or topical, regulated as to quantity, mode of abstraction, and repetition, by general principles of exigency. Under this view of the subject it will, therefore, necessarily follow, that in the

management of this formidable disease, all of successful or fatal issue will depend upon the judgment and discrimination of those who conceive and direct that management. Epidemic fevers are ever changing their type, with the changing seasons, of which they are the baneful and constant attendants: and therefore that return of the disease, which, at one season of the year, would require free depletion, will, perhaps, at another, either not admit of it at all, or at least will demand much caution and discrimination in having recourse to it. In the more unequivocal instances of inflammatory typhus, I need scarcely observe, that we shall have occasion to abstract a larger quantity of blood than under other circumstances. I can, however, but endeavour to establish the principle I wish to inculcate; leaving its proper and judicious application in detail, as referrible to circumstances of general or particular exigency, to those who are called upon to exercise it.

“ In that form of the disease which has been just alluded to, it has been often found necessary to repeat this measure until fifty or sixty ounces have been taken away. In most instances, however, we shall not find it necessary to go so far; provided we at the same time put into active effect other measures which may be termed auxiliary, but which contribute in a direct and powerful degree to make an impression on the complaint. These will be spoken of in their proper place. Several cases of inflammatory typhus have, within a short period, occurred to me, where the lungs, heart, and brain shewed indications of being so overloaded with blood, as that the patient appeared in the agony of death, with hurried respiration, purple colour of the lips, and the power of speech completely lost. These appearances were evidently the consequences of imperfect *reaction*, and were almost instantaneously relieved by blood-letting.

“ I need not remark that they occurred on an early day of the complaint. In three cases of this kind which I witnessed, the fifth, sixth, and eighth days were the periods of their occurrence; and the measure adopted was signally and unequivocally beneficial. These cases terminated within the fifteenth day. In other instances, of what at the commencement assumed all the appearances of inflammatory typhus, we sometimes find the symptoms yielding, *as it would seem*, to the usual palliative measures had recourse to on these occasions, and apparently assuming a mild character. The patient seems composed, and inclined to sleep. This latter appearance is apt to mislead those around him, and to give them expectations of a favourable issue. After continuing in this way for a few days, restlessness and want of sleep will come on, and slight convulsive motions *of the muscles of the face*, almost always a fatal symptom, will be observed. What was before conceived to be natural sleep, will now take on the character of confirmed *coma*, sometimes with convulsions; and the patient will die under all the appearances of effusion into the cavity of the head. I am disposed to think that in many of these cases, venous congestion takes place in the brain, as the consequence of great vascular excitement; and though acute or active inflammation

may not follow this inequality or imperfect circulation there, yet that effusion often follows. And even without the occurrence of such a consequence, I am of opinion that the cause of the result just alluded to, can be legitimately inferred from the admission of simple congestion in the veins and sinuses of the brain. Doctor Rush furnishes us, I think, with a satisfactory solution of this problem, by referring to cases of death under all the appearances of apoplexy, where, on dissection, no traces of these appearances could be detected, save unequivocal indications that the veins and sinuses of the brain had been much distended; the pressure from this cause produced the disease, and was sufficient to extinguish life, without lesion. The analogy, therefore, as applicable to certain states of typhus fever appears to me to be perfectly legitimate and apposite. In other instances of typhus I have seen a similar termination take place into the cavity of the chest, without the previous existence of those appearances which would warrant us in concluding that active inflammation of any of the organs of that cavity had existed. In these cases either the cellular substance of the lungs, or the pericardium, was the seat of the mischief, as appeared on dissection.

“In considering these effects as resulting from great vascular excitement, producing local congestion, what more probable means could be adopted for preventing their occurrence than such measures as would be most likely to make a direct impression upon their immediate cause; and it will not be inapplicable here to ask will wine and other stimuli effect this object? It may be said by the advocates for this side of the question that such terminations of typhus fever will result from *debility* and *relaxation*. I concede the premises; but here I submit that cause and effect are reversed in their application, for it appears to me, that no position in pathology is so gratuitously assumed, and, consequently, so precariously established, as this. I question much whether even the appalling sounds of *malignant* and *putrescent* can uphold it.

“Much discordancy of opinion prevails as to the time when blood-letting, either general or topical, should be had recourse to on these occasions, as well as to the quantity necessary to be taken. These questions, however, may, I think, be very easily decided, if we attend to what I have before said when speaking of inflammatory and congestive typhus. I apprehend no fixed period can be allotted to the propriety of having recourse to this measure, when symptoms requiring it may make their appearance. But it will be obvious, that in the advanced stages of the complaint, when vascular action has been allowed to continue for several days; and when, perhaps, its destructive effects have been increased or accelerated by the injudicious use of stimuli, until disorganization of some of the great vital viscera, inducing fatal *collapse* of the whole frame, has taken place, a period is marked, indicating the futility of this, or, perhaps, any method of treatment. I have seen cases where the abstraction of blood, both topical or general, was had recourse to with the happiest effects, as late as the fifteenth day; and I have seen other instances

when it could not with safety be ventured upon, in either form, at so early a period as the seventh. Doctor Mills, in his Treatise on Fever; seems to think that this measure should be limited to no period of the complaint. As far as my experience serves me I cannot go this length; but I may state my conviction, that in many cases of this complaint, where such a measure is supposed to be inadmissible by some physicians, it would be productive of the happiest results—here, however, the experience and judgment of the practitioner must decide for good or for evil.

“I am borne out in this remark by having seen several instances of typhus fever, which were attended by many symptoms of apparently a mortal character, yielding to natural hemorrhage; and this at a protracted period of the disease. A striking instance of this very recently came within my knowledge.—A young woman, whom I saw for the first time on the fifteenth day of her illness, appeared to be dying, under all the symptoms of what is termed putrid fever; coma, petechiæ, subsultus, tense belly, with great soreness there; hiccup, breathing difficult and laborious, extremities cold, and pulse quick, but still retaining some volume. On the evening of this day, considerable hemorrhage of dark-coloured, grumous blood, probably from the meseriæc veins, passed from the bowels, and the patient recovered rapidly. It has been said that hemorrhage under these circumstances cannot be adduced as argument in favour of blood-letting; because, forsooth, *Nature* is so wise and so jealous of interference, that she generally punishes those who have the hardihood to intrude upon her. Can any assumption be more probable than one which may be founded on the termination of the case just cited; or on similar ones, which could be adduced, and which are familiar to every physician. It is this—had active measures been used in these cases, for reducing great vascular excitement, and preventing undue distribution of the blood, is it not more than probable, that *Nature* would have been spared the effort she was obliged to make to restore her lost dominion? The answer is obvious. But I would go farther still, and say, that even at that advanced period of the complaint, in the instance just mentioned, *moderate venesection* would probably have been productive of the same effects as the natural discharge was. How often do we find in the last stages of hydrothorax, when the patient is almost suffocated, from congestions in the lungs, and consequently in the heart, that eight or ten ounces of blood taken from the arm, will give immediate relief; and though the change may be but temporary in this instance, the force or validity of the exemplification is not in any degree lessened. Thus then stands the view I take of the great and leading measure in the treatment of certain modifications of typhus fever. By referring to the observations I have put forward on the nature of its pathology, it will be seen, that I look upon the brain and its investing membranes, as those points to which I consider its force and malignity to be most frequently and formidably directed. Under this conviction, therefore, my first and most vigilant attention is ever directed to this great and leading organ

of vitality and sensibility, in the treatment of typhus fever, in all its modifications.

“ After the necessary depletion, therefore, either topical or general, as the case may require, I have ever found the application of cold to the shaved head signal and striking in its beneficial results. The forms I use are solutions of the muriate of ammonia in water with acetous acid-camphor mixture, and alcohol, or, equal parts of sulphuric ether, and rose water. I direct them to be kept continually applied to the head by means of a fold or two of linen or calico, saturated with the lotion every fifteen minutes. After proper depletion, sleep will almost invariably follow from the use of this measure; and that general tranquillity will prevail, which gives abundant evidence that an inflammatory state of the brain, or its investing membranes, is the cause of that tumult and distress which we so frequently find pressing upon us in this stage and form of the disease.

“ With respect to the use of other remedies which may be adopted in conjunction with blood-letting, as necessary towards the successful treatment of inflammatory typhus, the use of purgative medicines should first be considered. As remedies capable of making a direct and immediate impression upon so sensible and extensive a surface as the alimentary canal, it is obvious, that where the reduction of vascular excitement is our first aim, much will be effected through the agency of this class of medicines, actively and judiciously employed. Where depletion precedes their use, it will be found that their effects will be much more speedily and more decisively produced. This observation appears to me to apply equally to sudorific medicines. In acute rheumatism, or other diseases requiring their use, it will be found, that given in the largest doses, we shall often fail in producing their peculiar effect; but let twenty ounces of blood be taken from the arm, and we shall shortly have the patient inundated. It would, therefore, appear that the direct remedial powers of some of our most useful medicines will often depend upon the assistance of collateral agency.

“ Where depletion, in the cases under consideration, precedes the use of purgative medicines, I am of opinion that they are much less apt to cause that tympanitic state of the abdomen, which we so frequently see in this disease, and which gives rise to so much danger and distress. The cause of this result, I imagine, is, that when this class of medicines are trusted to alone, for the reduction or removal of great febrile excitement, they are used with much and injudicious freedom, *to the neglect of other and more appropriate means*: and that thus, from the long-continued irritation which they keep up on an extensive and exquisitely irritable membrane, as is the mucous coat of the alimentary canal, they very frequently superinduce a new and dangerous febrile action, *while they leave the original cause of disturbance in the system unreached and unmitigated*. Under the conviction then of their inefficacy, or obvious mischief, when trusted to alone in such a state of the system,

I unreservedly coincide with Dr. Graves, in the sound and judicious limitations he puts upon their use. Indeed I know no authority, ancient or modern, who more clearly sees his way in treating this formidable disease, than does that acute and able physician. During the prevalence here for these last two or three years, of typhus fever, *drastic purgatives* were almost inadmissible. I could cite very many instances of their injurious or fatal effects; and I would, therefore, warn the young and inexperienced practitioner against their free and *injudicious* use in this disease.

“In producing a desired and beneficial effect from the use of purgative medicines in typhus fever, we shall never be disappointed by calomel and rhubarb, followed by castor oil; or by infusion of senna with sulphate of magnesia, or tartrate of potash and soda. Added to these too, we have another effectual and most valuable resource in the use of *enemata*, in any strength or combination we may deem necessary.

“In many heavy and protracted cases of typhus fever, I have known the union of a mercurial alterative, the hydrarg. c. creta, with a few grains of rhubarb, repeated for two or three successive days, after depletory measures had been carried into effect, supersede the necessity for any other aperient medicine, during the remainder of their course. Under all the varieties of typhus, this combination as an aperient, is most useful; because, from its influence on the hepatic system, it tends to keep that great and important circle of organs free from congestion.

“Amongst the medicines used as purgatives in typhus fever, perhaps calomel holds the first place. That, under active excitement, given in full doses, either alone, or combined with other agents of this class, its effects will often be decisive in arresting the progress of general fever, and restoring the balance of the circulation when disturbed, or inclining to be so, no person can deny. In tropical climates this is a great and powerful weapon in the hands of the physician; and will not *undue* distribution of the blood, arising from general fever in the human frame, be regulated by the same laws, and moved by the same agents here, as there? The peculiar power, however, which calomel appears to possess, under these circumstances, is not by any means confined to its purgative qualities. By equallizing the circulation, and thus removing congestions from the various organs upholding life, we shall often find its powers almost incredible. In many instances of inflammatory typhus, it will be found that when mercury reaches the gums, the symptoms which before were considered of the most formidable and alarming nature, will now assume a mild aspect, and will yield to its influence in rapid succession. Respiration, which was before laborious, will now become natural; the bowels, which were torpid, will be easily moved; the skin, which was parched and intensely hot, will now appear soft and moist; and the delirium, which before was almost frantic, will, under this influence, settle into tranquillity and quiet.

“I am disposed to think that the production of the constitutional

Effect of calomel is powerfully influenced by the state of the vascular system. Under the influence of high excitement, when the pulse is full and hard, I have found it difficult to affect the gums; but after venesection and other antiphlogistic measures had been adopted, salivation quickly followed.

“But I am also disposed to think, that, under the influence of the state of the heart and arteries just alluded to, the stimulant powers of mercury have sometimes a tendency to be productive of unpleasant effects. Whether, however, such consequences can be established as resulting from a directly stimulant quality in this active medicine, or that they follow immediately from the operation of the original excitement, unconnected with any other agency, is a question of very difficult solution, and one which can be decided in no other manner, than by the test of repeated and accurate observation.*

“With the view of producing either the purgative or alterative effects of calomel in the disease under consideration, full doses of it will be necessary, especially at the early stage of the complaint. Doctor Armstrong and others, particularly the practitioners of tropical climates, give it in *scruple* doses, so as to ensure a speedy effect. In the early stages of *inflammatory* typhus, I seldom gave it in smaller doses than ten or twelve grains, and repeat it according to circumstances, until I am satisfied of, at least, its full purgative effect. When its influence on the gums is the principal object in view, it has been recommended to combine it with opium, in order to prevent it from passing off by the bowels. Under such circumstances, and with such views, should the physician be satisfied of the absence of congestion in the brain, much good may result from the tranquillizing effects likely to follow from such a combination. But, under appearances of cerebral fulness, I am disposed to consider the use of opium in any combination as inadmissible, until that state shall have been removed.

“As far as my experience enables me to judge on this subject, I am not, however, disposed to look for the peculiar and specific effect of this medicine, namely, mercurialization of the system, with much anxiety in the successful treatment of the fevers of these countries. In pure and healthy subjects, labouring under the *inflammatory* form of the disease, after proper depletory measures, I have seen many cases in which the action of this powerful medicine was signally and decisively beneficial. In the opposite character of constitution, or temperament, I have again seen it as signally injurious. I would, therefore, hold out to the young and inexperienced practitioner, that the production of its ultimate and specific effect on

* “I am now of opinion that in these cases the strumous diathesis is very frequently the solution of this problem; and hence, that in the production of the *peculiar* effects of mercury on the system all our powers of judgment and discrimination must be exercised under such circumstances.”

the constitution, in his curative views of typhus fever, should be with him a measure of great caution and consideration; and recommend him to pause before he adds fresh materials to a fire already burning too vividly; before which, his boasted art will very frequently fall prostrate, and his most plausible theories be dissipated into air.

“To return to the use of opium. In that restlessness which takes place in the early stages of inflammatory and congestive typhus, and which is so frequently the prelude to fatal *coma*, the use of opium cannot be too forcibly reprobated; as it never fails to aggravate or confirm this dreadful and intractable symptom.

“Under certain appearances of typhus, opium will, however, be found an invaluable remedy. When restlessness and delirium are found to prevail, *unconnected with fulness in the blood-vessels of the brain*, or in the pulmonary system, we may fairly attribute their existence to that state of nervous irritability by which many constitutions are so apt to be influenced by the excitement of typhus. Here, the beneficial effects of opium will be signal and striking. Hyosciamus will sometimes fulfil this indication very well too; but this narcotic will frequently fail when opium would succeed. In administering either of these remedies, however, where the slightest doubt is entertained as to the cause of the pressing symptom, the combination with the narcotic of a small portion of tartar emetic, the eighth or tenth of a grain, will be safe and prudent; and in this case the medicine should be repeated until its sedative effect is obtained.

“Doctor Graves's views on the utility of this combination are very clear and specific. I have been in the habit of using it for many years in the treatment of certain stages of fever with great satisfaction to myself; and I, therefore, look upon it as a most important addition to our curative means in this disease. I know no remedy the operation of which is less practically understood, and consequently more abused in the treatment of typhus fever, than is the indiscriminate and uncombined use of opium. If then a combination with it, which shall disarm such a weapon of its deadly and dangerous powers, and which shall yet neither retard nor diminish, but rather increase its soothing and salutary effects, can be found, such, assuredly, is a discovery beautiful in the contemplation of its acknowledged, though perhaps somewhat mysterious operation, on the animal economy under certain states of disease, and of the utmost importance to mankind in its results. In the purer states of inflammatory affections of the brain and its membranes; connected or not with the influence of contagion; and in active inflammations of the pulmonary substance and its tissues, the use of tartar emetic is, I believe, beyond comparison superior to all other remedies, in consecutive combination with proper and necessary depletion. In these cases its effects are frequently instantaneous and decisive, and we constantly see it interposing between the doomed objects of these

fatal states of disease, and that death, which, without such interposition, would inevitably await them.

“For the alleviation of delirium, and with a view to procure sleep in certain stages of typhus fever, I have frequently known warm fomentations to the head of great benefit. There is now residing near this city a gentleman in whose case this measure was signally beneficial. When living in a distant part of the county, some twenty-five years ago, after exposure to cold in the pursuit of field sports, he was attacked by fever. It ran its course in the usual manner, to the extent of twenty days; about the fifteenth day, delirium, subsultus, restlessness, and total want of sleep, became urgent and alarming. All the ordinary measures for their relief were unavailing. A flannel bag filled, with chamomile flowers, was rung out of boiling water, to which vinegar was added and laid on the head, which had been already blistered. In less than an hour tranquillity was produced, sleep very soon followed, and no fears were afterwards entertained for the result. Many similar cases have occurred to me where the same measure was productive of great benefit. In two of these, where the symptoms were most pressing and alarming, the flannel bag containing the chamomile flowers was wrung out of a quart of boiling water holding in solution a drachm of powdered opium. The effect in both instances was striking and happy.

Camphor has been very extensively, and, I fear, injudiciously used with a similar view; but its effects are in many instances directly contrary to our expectations; and at all times, it is a medicine of uncertain efficacy. After depletion, indeed, it will sometimes be found beneficial in allaying irritation; and in these cases, combined in small doses with James’s powder, it will frequently be productive of much benefit.

“I need not here dwell on the use of cold affusion in the incipient stages of typhus. The particular circumstances which should regulate its use are known to every physician. In the purer states of inflammatory typhus, when had recourse to at an early period of the disease, much benefit may be derived from it. Its salutary effects appear to me to depend solely on its power of equalizing the circulation of the blood, and thus removing or preventing determinations to the internal organs. This idea is supported by its being ascertained, that the beneficial effects resulting from its use are directly proportioned to its action on the perspiratory organs. Temporary reduction of morbid heat will always follow from its action on the surface of the body; but in arresting the progress of fever, perspiration is essentially necessary to render its operation decisive. In cases of inflammatory typhus, the cold affusion, used after early depletion by the lancet, would appear to be a valuable addition to our curative means.

“In the congestive form of typhus, the more particularly when it speedily assumes its peculiar characters, I doubt much, whether the use of this remedy would not be precarious, if not hazardous. In these cases the brain is so soon, and so powerfully affected, that *reaction* may not follow its use. Under such circumstances death

would be the inevitable consequence, because it would only confirm what it was intended to remove.

“The use of the warm bath in typhus fever has been too much overlooked. In conjunction with the means already spoken of, great and decisive advantage will be derived from it. United, therefore, to other depletory measures, warm bathing, by allaying nervous irritation, and determining to the surface, will be found a valuable and powerful adjuvant in reducing excitement.

“Under the *sub-acute* form of inflammatory typhus, we shall frequently have to contend with symptoms equally formidable in reality, though, perhaps, not apparently so, as in the more acute species of the disease. In this species of typhus, the tendency to inflammation is very generally directed to the various *membranes* of the body. Hence, we frequently find the pleura, dura mater, arachnoid membrane, and peritoneum in various parts of its extensive distribution, evidently labouring under well-marked appearances of inflammation.

“In these cases, the pulse will most commonly be quick, small, and hard. If the membranes of the brain should be its seat, there will be headach, and early delirium; and sometimes intolerance of light, restlessness, and total absence of sleep, with, occasionally, slight subsultus, will be present from an early period. The temperature of the body will not be as high as under other modifications of the complaint. Should the *pleura* be the seat of this influence, respiration will be hurried, and cough, with slight pain or soreness of the chest, or sides, will be present. Where the peritoneum suffers, to any extent, the whole cavity of the abdomen will be more or less effected with pain and tenderness on pressure. The bowels will be generally confined, and the evacuations will be dark-coloured.

“Numerous instances of this form of typhus have lately occurred to me, and I can candidly assert, that every case which I treated with blood-letting, either topical or general, had a favourable termination, though many of them assumed a very alarming appearance. When such appearances take place, without much fulness or asperity of the pulse, leeches, freely applied near the seat of the affection, will be followed by the happiest results. Saline purgatives too, under these circumstances, are very useful. I have now under my care a young lady, who is recovering from this form of typhus, who was on the eighth day of her illness seized with low peritoneal inflammation of one of the intestines, which was effectually removed by the application of twelve leeches over the seat of the affection, which could in this instance be distinctly traced to the arch of the *colon*.

“This patient could not be prevailed upon to take neutral salts. As an active substitute, a combination of calomel, aloes, and hyosciamus was had recourse to, and repeated four times. The alvine discharges following each dose of this medicine were copious, intensely black, and consisted almost entirely of scybala. In this case the tongue was coffee-coloured, and the whole body covered with *petechiæ*. No wine was given up to this day, the seventeenth of the disease.

“ As a powerful auxiliary resource, warm bathing, or where this cannot be conveniently carried into effect, local fomentations will be productive of much benefit in these cases. Under aggravated states of this particular form of typhus, obstructions will frequently form in the bowels, and great tension and soreness will follow, attended by irritability of the stomach, with small quick pulse. In two instances of this kind of which I had the management within this last month, leeches were applied to the abdomen, and a draught consisting of equal parts of castor oil and spirits of turpentine, was afterwards given. A full purgative effect speedily followed, with almost immediate alleviation of all the symptoms in both instances. In that state of irritation of the villous coat of the intestines which we so frequently see in typhus fever, I think I have seen much benefit to arise from small doses of the ol. Terebinth: ten or fifteen drops occasionally on barley water. This state too is frequently combined with a very diminished secretion of urine, and here turpentine often acts happily. On these occasions also, as well as in that distressing state of tympanitis which has been before alluded to, when the villous coat of the intestines, or the peritoneum, in various points of its distribution, suffers from a state of *sub-acute* inflammation, I have found the external use of turpentine by friction, attended with the most beneficial results. In these states I direct it to be rubbed warm, by means of a piece of flannel saturated with it, over the entire surface of the abdomen for a minute or two, and repeated every three hours, until its specific effect—sensible and decisive irritation—shall be felt. Blistering the surface of the head or other parts, where congestion may be disposed to take place, is frequently a measure of much efficacy; and regulated by those rules and indications, which derive their origin from a sound pathology, I believe it will very frequently be productive of signal benefit. But I am disposed to consider it a means of relief greatly overrated and often misapplied in the treatment of typhus fever. Doctor Graves, when adverting to it, in his admirable clinical lectures, appears to consider it almost the leading measure of safety and efficacy in this disease. That he has applied it judiciously and successfully I unreservedly admit; and regulated by such a judgment as his, I feel convinced he will continue to derive satisfaction from its use. But I would respectfully suggest, that his reliance on it is sanguine beyond its success in other hands; within my own experience, particularly in the early stages of inflammatory typhus, I could, in numberless instances, contrast it, much to its disadvantage in efficacy, with cold applications to the shaved head. In certain stages of the complaint, however, when we are on the eve, or in the actual presence of that torpid state of the nervous and capillary systems, which we so frequently witness, I am firmly convinced that the application of this measure, as directed by Dr. Graves, will be signally beneficial. That learned and able physician, however, appears to consider it one of the most efficient *preventatives* of mischief in this formidable malady. I am rather disposed to look upon it as a very useful adjuvant to other measures;

founded, in my opinion, on sounder views of treatment in their pathological application, as well as of greater efficacy in their results.

“ I was about three years since called a considerable distance from this to visit an estimable gentleman labouring under typhus fever, with symptoms from its commencement, as I learned, of urgent determination to the head. My first visit—the eleventh day of the disease—found the entire surface of the head blistered effectually, with little advantage; at my next visit—an interval of three days—I found the head and neck frightfully and enormously tumefied, hot and red, throughout their entire extent, from the application of tartar emetic ointment. The patient died *comatose* on the following day. Did this powerful source of irritation increase, in this instance, the determination of blood to the head, and thus tend to produce or aggravate that mischief which it was designed to remove or prevent?

“ In considering the use of wine in the various modifications of typhus alluded to in the preceding pages, I am aware, that I enter upon a subject fertile in discord amongst medical men. But if the pathology of this complaint, which I have ventured to suggest, be allowed to rest upon what appears to me to be the only solid foundation which we can, in the present state of our knowledge allot to it, much of the difficulties hitherto found in settling this question may perhaps be removed.

“ From what I have already observed, it will be seen that I consider excitement of the vascular system as the first *active and ostensible* attribute of typhus fever; and that the whole train of subsequent symptoms, whether of mildness or severity, are derivable from this source. It will also be recollected, that I attributed to this impulse in numerous cases a distinct character, as taking its rise from contagion, or from a peculiar combination of exciting causes, which produce the complaint in question, but which, in many instances, cannot be assigned to the influence of contagion. If, therefore, we allow to excitement in these cases the character which it is admitted under all other circumstances of its operation on the animal economy to possess,—namely, the natural and unerring power of exhausting itself, and thus producing its opposite effects, we must, in my humble judgment, admit the force and propriety of assigning to this agent its proper and legitimate features under every modification of its existence. Those modifications, it was remarked, will be powerfully influenced by particular circumstances already alluded to; and hence, that the physician, in regulating his curative measures, must be guided rather by the effects which those measures may produce, than by attempting to establish fixed or definite rules for meeting the exigencies which he is so often called upon to encounter, either upon the consideration of their actual existence, or of their prospective occurrence.

“ It must be obvious, therefore, under these circumstances, that the state of collapse in typhus fever, when it occurs, will require different quantities of *stimuli* in different individuals. Doctor Arm-

strong has remarked, that we shall often find it necessary to have recourse to the use of blood-letting and *stimuli* at the same time; and this observation I have frequently seen exemplified in cases where topical abstraction of blood from the head was signally beneficial, *even in the advanced stages of typhus, under a state requiring rather a liberal use of wine.* The quantity of wine, therefore, or other stimuli, which may be necessary, in numerous cases of typhus, cannot be established by any arbitrary rules. In many individuals suffering under this complaint such a degree of *collapse* will follow, even from the mild form of the complaint, as to require very liberal doses of stimuli. In other instances such remedies cannot be administered, in any combination, without producing a certain and speedy aggravation of most of the symptoms, and, perhaps, inducing others of a more formidable nature. Where wine, therefore, may be thought inadmissible in these cases, from the tendency it so frequently shows (even under states of the system apparently favourable to its use) to cause hot skin, restlessness, and delirium, we shall find a safe and often a most efficacious substitute in the carbonate of ammonia, united with camphor mixture, and spirit. ether. nitros.—Many cases have come within my cognizance where this combination, given on the verge of collapse, has acted like a charm, bringing about, within a very few hours, tranquillity, sleep, moist skin, and copious diuresis,—the very essence and attribute of what has been designated *crisis.*”

The Stomach in its morbid States; being a practical Inquiry into the Nature and Treatment of Diseases of that Organ; and into the Influence they exercise upon the Origin, Progress, and Termination of Diseases of the Liver, Heart, Lungs, and Brain. By LANGSTON PARKER, Member of the Royal College of Surgeons, and Fellow of the Royal Medical and Chirurgical Society of London, &c.

MR. PARKER is already advantageously known to many of our readers by means of a paper “*On Stomach Diseases and their Sympathies,*” published in this Journal in September, 1835. That paper contains excellent practical observations, and proved that Mr. Parker possesses all the qualities necessary for a sound physician and an instructive writer. Since the publication of that paper our author has continued to bestow much attention on the nature and treatment of diseases of the stomach in general, and the volume, whose title we have given above, has been the result of his labours; the importance of the class of diseases which Mr. Parker has made the subject of his treatise, will be readily acknowledged by every practical physician; stomach diseases are of every day occurrence; they

form the national malady of Britain, and consequently the prime staple of the medical art. Innumerable as are the publications on indigestion, we have long felt that none had attained to the level of the present state of physiology and pathology; indeed it may be asserted with perfect truth, that in no department of medicine were errors, practical errors, more prevalent. Though much had been done by Philip, Johnson, Abercrombie, and others, yet but little was known by the generality of physicians of the researches of foreign authors; and the routine aperient and blue pill practice of Abernethy had still many advocates. Mr. Parker's work will be found well calculated to supersede the defective treatises on indigestion and diseases of the stomach, which have been published as monographs, or have been embodied in practices of physic and cyclopædias; our author does not seem to be led away by an undue partiality to his favourite subject, and he wisely avoids the fault so commonly committed by writers on the diseases of some particular organ, in seeming to think all other ailments either comparatively unimportant, or directly derivable from the maladies of the organ in question.

With writers on diseases of the stomach the stomach is every thing; heart, brain, nerves, &c., are merely secondary, and are besides entirely governed by the stomach, the ruling centre of the outward as well as the inner man. Physiologists too, when writing on digestion, and anatomists on the digestive apparatus, are apparently under the dominion of this fixed idea, this *monomania*. Thus what has modern discovery made out concerning the abstract idea of animal life—what is the nucleus, the vital centre of a human being? Many and glorious have been the dreams of philosophers and the theories of anatomists on this subject; the soul, the vital principle, mind, the brain and nervous system have occupied this station, and most physiologists have conceded to them the honour of being the objects for which all the other organs of the body were created, and to which they are all subsidiary; but now physiology points to less noble functions, and threatens a revolution in all our views, deposing the mind and brain from their high place, and substituting the stomach in their stead.

This is a consummation which destroys at once the *beau idéal* of animal existence, by making the stomach the essence of our being. Thus in an able article on the Digestive Canal, lately published in the Cyclopædia of Anatomy and Physiology, we have it announced on the best authority, that “an animal in the abstract may almost be viewed as a moving sac, organized to convert foreign matter into its own likeness, and all the complex organs of animal life are but auxiliaries to this *primitive*

digestive bag." Are we come to this at last! We, the lords of the creation, made after God's own image, we are nought but primitive digestive bags! endowed with the faculty of converting foreign matter into our own likeness, i. e. into bags! What an ennobling view of animated nature!

Mr. Parker has brought all the light of modern physiology to bear on diseases of the stomach, and has evinced much research in availing himself of the labours of his cotemporaries both in England and the Continent. His practical works are always valuable, and often original; Mr. Parker gives in the preface a general outline of his work.

"The work I now lay before the public," he says, "is materially different from all those which have preceded it on the same subject: it is neither devoted chiefly to the consideration of pathological changes, like that of Dr. Abercrombie, nor is it limited to one class of primary morbid states. The subject of organic disease, in itself, has never appeared to me so important as that of the primary conditions which precede it, which, by their continuance through a series of years, ultimately induce incurable affections, either in the organs where they are seated, or, by sympathy, in remote parts. In a practical point of view, the latter is of infinitely greater importance than the former.

"It is from this circumstance that I have devoted the following treatise principally to the consideration of the primary morbid conditions of the stomach, and the diseases they induce, by sympathy, in remote parts, as the liver, lungs, heart, and brain. The primary morbid conditions of the stomach may be referred to two classes: 1. congestive or inflammatory states; and 2. affections of its sensibility, both organic and animal. There is a third form—a disordered state of the secretions, which I must denominate primary, though it is not really so: we cannot, however, appreciate any pathologic condition which precedes it.

"After having noticed these conditions of the stomach, their symptoms, and mode of treatment, I have passed to the consideration of their influence upon the origin, progress, and termination of diseases in other organs. The question of morbid sympathy is one of extreme importance; and, as far as the present subject is concerned, I am not aware of any author, with the exception of Dr. James Johnson, who has more than touched upon the sympathies of the stomach with internal organs. I have here endeavoured, in some measure, to illustrate this influence; convinced that the primary morbid states, to which I have alluded in the earlier parts of this work, are daily the sources of disease in remote internal organs, which ultimately, by their continuance, terminate in organic change."

The volume, containing 303 pages, is divided into thirteen chapters, as follows:—

Chap. 1.—On the morbid states of the stomach characterized by increased vascularity.

Chap. 2.—On the morbid states of the stomach dependent upon anemia.

Chap. 3.—General review of the symptoms and sympathies dependent on vascular irritation of the stomach.

Chap. 4.—Of confirmed inflammatory affections of the stomach.

Chap. 5.—On affections of the stomach characterized by derangement of its sensibility.

Chap. 6.—On affections of the stomach characterized by morbid states of its secretions.

Chap. 7.—On the influence of the stomach upon other organs.

Chap. 8.—On the influence of morbid states of the stomach upon the origin, progress, and termination of diseases of the liver.

Chap. 9.—On the influence of morbid conditions of the stomach upon certain forms of dropsy.

Chap. 10.—On the influence of the morbid states of the stomach upon the origin, progress, and termination of diseases of the heart.

Chap. 11.—On the influence of morbid states of the stomach upon the origin, progress, and termination of diseases of the lungs.

Chap. 12.—On the influence of morbid states of the stomach upon the origin, progress, and termination of diseases of the brain.

Chap. 13.—Of the treatment.

We have carefully perused the chapters above enumerated and we can affirm with truth that no modern work appears to us, superior in sound and practical information; we regret that we cannot present our readers with an analysis of the whole; but we think we shall be rendering them a special service by communicating without abridgment the last chapter on the treatment.

“The treatment of diseases of the stomach may be divided into two grand sections—dietetic and medicinal. Of the former enough has been said in the writings of Paris, Johnson, Wilson, Philip, Abernethy, and others, to render a recurrence to it here unnecessary; the latter must be framed to suit the particular group of symptoms manifested by different individuals; and for this purpose, we shall distribute the symptoms into several classes, taking the predominant symptoms which demand the chief attention as the type of disease in the class to which it belongs.

“ 1. *Of the Treatment of those Forms of Stomach Disease whose prominent Symptoms are Pain and Constipation.*

“ In the affections of the stomach which are characterized by these two predominant symptoms, we have internal pain of a more or less violent character, occurring at various periods after a meal, which sometimes increases to such an extent as hardly to leave the patient free for an hour, the pain occasioned by one meal not having subsided, before it is again called forth by a second. These forms of disease are commonly accompanied by constipation, which invariably aggravates the patient's state; and yet the common forms of aperient remedies cannot be borne, on account of the great additional disturbance they create. I will detail a case as a type of this class of symptoms:—A lady, aged forty-two, had been subject to attacks of pain after food for fifteen years, they were now increased to such an extent that she was never free from distress, mixed food produced agonizing pain, and even thin cold gruel occasioned considerable uneasiness. The bowels were obstinately confined, she passed three or four days without an evacuation, which was then only artificially induced; the tongue was red and smooth, the pulse frequent; the epigastrium was scarcely sensible to pressure. She had emaciated much during the two months previous to my attendance. Leeches and counter-irritants were used to the epigastric region without much relief, but the patient lost all pain, the bowels were relieved, and in a few weeks she was completely established by confining her to farinaceous and milk diet, and giving the following medicines. *R Pulv. rhœi gr. iv., morphiæ muriatis $\frac{1}{12}$ M. ft. pil. ter die sumend.; c. cochlear. iij. larg. misturæ sequent. R. Infus. cascarillæ \bar{z} vii., magnes. sulphatis \bar{z} ß, magnes. carb. pond. \bar{z} iß, tinct. aloes, \bar{z} ss. acidi hydrocyanici \mathfrak{m} xv., tinct. humuli \bar{z} ii. M. capitat cochlear. iij. larg. ter die.* These medicines acted freely, without occasioning pain or any uneasiness. They were employed by the patient for three months with the greatest benefit, occasionally increasing the quantity of morphia.

“ I have not found material benefit from leeches in such forms of disease as these, nor generally from counter-irritants. I believe the best of the latter that can be employed are blisters, powdering the denuded surface of the latter with a grain or more of the acetate or muriate of morphia. I have seen a number of cases of constipation of this kind, which have been aggravated by drastic and cold saline aperients, yield almost magically to the combination I have just mentioned. Enemas had also been totally inefficient in relieving the constipation. These forms of disease appear dependent upon a slight inflammatory affection of the stomach occurring in patients of great nervous susceptibility; sometimes they are accompanied by tenderness in the epigastrium, a red, loaded tongue, frequency of pulse, and evening accessions of fever, whilst at others, the tongue is pale, the pulse small, but not frequent, the skin cold, and tenderness in the epigastrium entirely absent. In the former of this class of symptoms, I should consider the disease one of sensibility, combined with

a trival degree of inflammatory action ; in the second, a disease of sensibility alone. To the former of these diseases, Barras has applied the term “gastro-enteralgie avec la gastro-enterite chronique :” certainly, in many cases, the symptoms of inflammation are decidedly present, whilst in others the disease of sensibility exists without this complication. In the former state small local depletions are of service, but, if the loss of blood be large, the nervous symptoms are invariably aggravated ; three, four, or six leeches, applied from time to time, are of great service ; a larger number frequently increase the pain, and add to the debility of the patient, which, if the disease have continued a long time, is in many cases very great. The constipation should be relieved by the form of remedy I have given, or a similar one. The following I have found of great service, leeching, or blistering, the epigastrium at the same time, if tenderness be present.

“℞ Magnes. carb. pond. ʒi., bismuth subnitrat. gr. v., morphiæ muriatis gr. $\frac{1}{8}$, *m* ft. pulvis ter die sumendus.

“This is chiefly useful after the constipation has been removed. After the bowels have, by appropriate treatment, been brought to act well without medicine, and the whole of the symptoms of inflammatory disease have subsided, a combination of the muriate or the acetate of morphia with the carbonate or sulphate of iron ; or the syrup of morphia with the muriate or the ammoniated tincture of iron, will generally be found serviceable in preventing the recurrence of pain.

“The indications under this head are to remove pain, to obviate constipation, by which it is invariably aggravated, to subdue concomitant inflammatory action, and to enable the stomach, when these intentions have been accomplished, to fulfil its offices again properly.

“2. *The Treatment where Vomiting and Diarrhœa are the predominant Symptoms.*

“In many cases these symptoms exist together, or vomiting comes on occasionally, when an habitual state of relaxed bowel is constantly present. In other cases, vomiting may be present with constipation, or diarrhœa may exist without vomiting. In the great majority of instances both these symptoms are dependent upon a chronic irritation of the gastro-intestinal mucous membrane, of the inflammatory kind. It is a common circumstance to find persons indulging in the pleasures of the table, with bowels constantly relaxed, at the same time they have total loss of appetite, whilst the tongue is foul, the papillæ elevated, and the front and edges of the organ vividly red. At times headach or thirst is added to the other symptoms. The most simple aperient in many of these cases will produce profuse evacuation. I have noted many of these cases continuing for months together. Suddenly the symptoms of acute gastritis have been manifested, and the epigastrium, which was not be-

fore tender, has become so sensible that the slightest pressure could hardly be borne.

“ A gentleman, aged forty-eight, had suffered from this state for many months, his bowels were habitually relaxed, two or three loose evacuations daily; a small dose of rhubarb or magnesia produced six or seven stools, he had no appetite, and a loaded tongue, vividly red at the point and edges. He had not the least epigastric tenderness. At times severe vomiting, with increased diarrhœa, would come on, unattended by pain, in which state he found relief from a combination of blue pill with opium, and the mist. cretæ with conf. opii, and hydrocyanic acid. These symptoms suddenly assumed an acute form, he had fever and acute pain in the epigastrium, pressure in this region could hardly be borne, whilst every thing taken into the stomach produced most severe pain. These symptoms were subdued by daily relays of leeches, at the same time the patient took internally the hyd. c. creta with pulvis ipecac. co. and the hydrocyanic acid in almond emulsion. Under this plan the appetite became good, the tongue clean, and the bowels lost that disposition to irritability which they exhibited before the appearance of the acute attack.

“ In all cases of this kind, the great point in the treatment is to allay the irritability of the mucous surfaces by mild opiate, antacid, or absorbent remedies; no active measures should be resorted to, they invariably aggravate the patient's condition; a mild, unstimulating, nutritious diet, consisting of milk and farinaceous aliments, with such medicines as the hyd. c. creta, with p. ipecac. co., two grains of the former with one of the latter for a dose, the carbonate of soda with morphia, if much acidity be present, or a grain or two of rhubarb with the same anodyne, are the best and safest remedies to be employed; the hydrocyanic acid also may be given with these remedies in the mistura cretæ. Should the epigastrium become tender, it must be leeches; after the repetition of local depletion two or three times, if the tenderness continue, without much heat of skin, blisters may be employed; and the surface powdered with morphia, or dressed with an ointment containing three or four grains of this salt to the ounce.

“ It is this form of gastro-intestinal irritation which so frequently precedes the development of hepatic and pulmonary diseases. Andral has noticed the tendency to irritation in the lungs of patients so affected. I have collected the cases of several individuals, who seem disposed to hepatic diseases, and who, if these irritations are suffered to remain unchecked, speedily become jaundiced.

“ 3. *Treatment of the more acute Forms, characterized by great Epigastric Tenderness and Irritability of the Vascular System.*

“ When we find the more acute forms of gastric inflammation, arising in persons predisposed to great vascular excitement, with increased heat of skin, accelerated pulse, throbbing of the heart, and giddiness after a meal, continuing during the whole period of digestion, and accompanied by great sensibility of the epigastrium, our

first treatment must consist in the local abstraction of blood from this region; this should be continued daily in small quantities proportionate to the strength of the patient, whilst any heat or tenderness remain, or whilst the arterial system is excited by taking food. In the intervals, the stomach should be covered by a fomentation of hops, poppies, or an aqueous solution of opium, whilst we limit the patient to a strict dietetic discipline—tepid gruel, or thin farinaceous food; and give him internally such remedies as the following:—
 R Acid Nitro-muriatici M. xl., morphiæ muriatis gr. $\frac{1}{2}$ to gr. i., syr. simp. $\bar{3}$ i., aquæ distillatæ $\bar{3}$ vii., M. capt. cochlear. iii. larg. 4tis horis. These are exactly the forms of gastric inflammation, which Broussais has taken as a type of the whole series; and here we agree with him that aperient medicines are decidedly hurtful, not only augmenting the tendency to irritation of the stomach itself, but also increasing the vascular excitement, and disposition to the occurrence of sympathetic affections of other organs, as the heart and brain.

“A gentleman, about forty-five years of age, had suffered from vomiting, flatulence, and fulness after his meals, for some time, with tenderness and great heat in the epigastrium, acceleration of pulse, throbbing of the carotids, palpitation, stupor, giddiness, and inability to think after having eaten. The daily application of leeches to the epigastrium relieved the symptoms to convalescence; at the same time the patient took internally the mixture prescribed above. In this case a teacup full of food (even gruel) brought on the symptoms in their usual form. There are two remarkable points illustrated by this case; the inflammatory form of a great majority of cases of simple indigestion, marked in the first instance by mere fulness after a meal, which was the primary symptom in this case; and the great importance of limiting the patient, in such diseases as the present, to just so much food as may be digested with comfort, without stimulating the mucous coats of the stomach to a degree which may excite the heart or arterial system. In such forms of disease as the present, the mode of local bleeding proposed by Roche might be adopted with great benefit—i. e. to apply leeches to the epigastric region, at the periods only when the febrile or arterial excitement comes on; for, in many instances, this is only manifested after the periods of taking food. In the intervals, the patient is comparatively well; hence, the great point in the treatment is to diminish the excitability of the stomach to impression at the time when food is taken into it. Dr. Roche was of opinion that the inflammatory condition of the stomach, and the irritability consequently manifested, was not much relieved by bleeding in the intervals of the accessions of vascular excitement, which were manifested, as in the present instance, after a meal.

“4. *Treatment where Fulness, Distention and Acidity, with Flatulence and Eructations after eating, are the predominant Symptoms.*

“To this class of symptoms the term indigestion is most frequently applied. These symptoms mark a condition of the stomach

in which active hyperemia, or morbid fulness of blood, not amounting to inflammation, is the pathologic character of the disease. This is the first step to chronic gastritis, but it is not the disease. In these forms, when the stimulus of food is absent, the coats of the stomach return to their customary state, and the patient is well till another full meal brings on again the congested condition of its mucous membrane. Taking this condition of the stomach as one of its primary morbid states, we may see how easy is the progression into confirmed chronic inflammation, and its termination in changes of colour and consistence, softening, ulceration, or cancer. The treatment of this form of disease must depend, in a great measure, upon the addition of other symptoms to the predominant ones, just now detailed, such as pain after food, epigastric tenderness, diarrhœa, or constipation. If the epigastric tenderness be not marked, and the bowels, as they commonly are, full and confined, we may commence the treatment by removing the use of stimulants from the plan of alimentation, and limiting the patient to milk and easily digestible food. It is here here that aperients, judiciously administered, are of great service; but we must bear one thing in mind in their exhibition, viz., that the stomach and bowels being in a morbid state, are particularly sensible to impression, in these forms of disease; and hence the administration of purgative medicines, although clearly indicated and properly chosen, is sometimes followed by a degree of relaxation of the bowels approaching to hypercatharsis. I have seen this state produced over and over again, when the symptoms of hyperemia of the stomach were present, and the bowels at the same time confined. For the same reason does digitalis produce vomiting when exhibited in this state of the stomach, complicated with disease of the heart. Stimulant diuretics in dropsies or hepatic diseases are productive of the same evils, where the state of stomach is present. The form of aperient remedies I am generally in the habit of employing in these states are the following:—*R.* Pil. hyd. gr. ii., pulv. rhœi gr. iii., ipecac. co. gr. i., mucilaginis q. s. ft. pil. ii. These may be taken twice or thrice a day, with two or three table spoonfuls of the mixture prescribed in the first section. If the flatulence be distressing, a combination of the pil. hydr. with the pil. galbani co. is of great service. At other times, the blue pill may be used with the extract of hyoscyamus. These remedies should be given with different proportions of the mixture, according to the circumstances of the case. I have found great benefit from exhibiting the pills and mixture one hour before dinner, once in the day, and repeated before the evening meal, if the bowels require it.

“If much tenderness and pain exist in the epigastrium, if the pulse be full and hard, the tongue red and dry, small relays of leeches will be of great use. I have frequently cured these indigestions by this remedy alone; four or six leeches, every two or three days, applied during the period of digestion, requesting the patient, at the same time, to abstain from all stimulating aliments, and to live upon thick-

ened milk, sago, or farinaceous food, substituting cocoa for coffee or tea.

“ Much stress is laid by the French physicians on the use of mucilaginous and acidulated drinks during active hyperemias, or inflammatory states of the mucous coats of the stomach. This is a treatment which is too much neglected in this country, as well as that of anodyne fomentations to the epigastrium. The experiments of Dr. Beaumont* show that, during fits of repletion or of gastric derangement, from over-stimulating or other causes, or during feverish states of the constitution, the mucous coat of the stomach becomes dry and covered by red patches; this is the state that may be supposed to occur two, three, or four hours after a full meal, the period of its occurrence being marked by thirst. This condition of the mucous coat is also present during ordinary fits of indigestion, and hence the propriety of defending the sentient extremities of the gastric nerves from the rude impression of alimentary substances, when the stomach is in this state, by mild tepid acidulous or mucilaginous drinks, taken in small quantities. Broussais assures us that he has frequently prevented the passage of mere indigestion, of the inflammatory form, into confirmed gastritis, by exhibiting small portions of drinks of this character during the accession of the fits of indigestion after a meal. We know that the sentient extremities of all nerves, throughout the economy, must be kept in a moist state, in order that they may be enabled to fulfil their customary functions. For this purpose, we find the extreme branches of all nerves covered by particular fluids. If these fluids be dried up, the nerve no longer transmits a true impression to the brain, and its organic, as well as its animal sensibility becomes destroyed, or morbidly excited. Itard has attributed one species of deafness to absence of the fluid in which the extremities of the auditory nerves are distributed, in the semicircular canals and cochlea, in the internal ear.

“ I do not think that revulsives, such as blisters, the tartar emetic ointment, or plaister, or frictions with croton oil, are generally beneficial in affections of the stomach of this character, in fact in diseases of this order generally, unless the disease be perfectly apyretic. I have seen them, if applied whilst any vascular excitement is present, productive of great mischief.

“ The outline of the treatment, given by Dr. Stokes, of chronic gastritis, in the article of that name in the *Cyclopædia of Practical Medicine*, is well suited to the incipient forms of inflammatory indigestion. This consists in small local bleedings, diet, and sedatives, such as the salts of morphia, and hydrocyanic acid. The combination of these two latter remedies, in various forms, with mild aperients, is, however, of singular use in various forms of disease of the stomach. I have given some forms of combination in which these

* *Experiments and Observations on the Gastric Juice and the Physiology of Digestion.* Plattsburg, 1833.

remedies may be employed. Dr. Chauffard, of Avignon,* employs chiefly, in these forms of indigestion or gastric hyperemia, small bleedings, with anodyne or acidulous drinks. He appears to have been very successful in his treatment of the inflammatory forms of indigestion.

"I shall now offer a few remarks on the principal remedies employed in diseases of the stomach.

"*Bleeding, general and local.*—General bleeding in diseases of the stomach, even of the inflammatory kind, is inadmissible, except perhaps in very severe forms of acute gastritis, where a single bleeding might be employed at the commencement of the treatment, prior to local depletions from the epigastrium. The testimony of most authors is against general bleeding. Broussais was singularly unsuccessful in its employment, even in cases of acute disease, which we shall see by reference to his cases thus treated.† Local bleeding is, on the contrary, one of the most efficacious remedies that can be employed in all affections of this kind. It should not precede the use of other remedies, but when these seem to be productive of little benefit, or mere temporary amendment, judicious local depletion from the epigastrium should be employed. This remedy may be resorted to in all cases where fulness of blood in the mucous coat of the stomach is suspected, from whatever cause it may arise; even in cases of extreme emaciation from long continued disease, two, three, or four leeches will sometimes alleviate symptoms which no medicine will benefit. In cases of acute inflammation, leeches may be employed freely to the number of fifteen or twenty at each time. If, however, acute or sub-acute gastritis arise during the progress of an affection of the heart or the liver, the depletion must be more cautiously employed, recollecting that we have to deal with a constitution already enfeebled, or rendered irritable in consequence of long continued disease. In cases of confirmed chronic inflammation of the stomach, more benefit will be derived from the daily application of small relays of leeches to the number of six or eight, than from one large depletion where more are employed. I consider the quantity of blood taken at a time in cases of this kind to be of extreme importance. Unpleasant nervous symptoms very commonly accompany diseases of the stomach of the inflammatory kind, and a trifling degree of inflammation sometimes produces an affection in which the nervous symptoms predominate over the inflammatory; hence local depletion, although of vast utility, should be employed with care and caution, for it is a very common circumstance to see large depletions of this kind succeeded by an aggravation of the concomitant nervous symptoms. The large local bleedings resorted to by Broussais, and the physicians of the French physiologic school, have been productive of infinite mischief. If we peruse carefully the work of Barras, *Des Gastral-*

* *De la Saignée et des Emolliens les Indigestions.*

† Cases 5, 6, 7, art. Influence of the Stomach upon the Lungs.

gies et des Entéralgies, we shall be convinced that many of the cases related by him of nervous affections of the stomach and bowels were the result of large local bleedings, the use of fifty, sixty, or even eighty, and a hundred leeches at each application. Many of the examples reported by M. Barras, which are considered by him merely nervous diseases, and which were so at the time he was consulted, requiring nothing but full diet and steel for their cure, were, in the commencement, disease of the inflammatory kind, the patient having been rendered irritable and nervous by the large bleedings and rigid abstinence employed at the commencement of the treatment. De Larroque, whilst he illustrates the utility of moderate depletions from the epigastrium in inflammatory diseases of the stomach, irritating the lungs, loudly exclaims against the large local bleedings practised by many of the physicians of the physiologic school. The period of bleeding is also of importance. Many forms of gastritis are strictly periodical; in others the prominent features of the disease, as pain and distention of the stomach, heat of skin, and quickened circulation are only present after meals. It is during these accessions that bleeding should be employed; it is at these periods very much more useful both as a palliative and curative remedy, both in producing present relief, and preventing the recurrence of future attacks, than when resorted to at other times in the intervals of the exacerbations of disease.

“*Aperients*.—Many physicians appear to regard all gastric irritations as mere suburral states, and consequently employ strong drastic aperients for their cure. By others, all aperient remedies are condemned, as likely still more to excite a membrane already reddened by irritation. I have shewn that there are many forms of stomach disease, in which constipation is a prominent and distressing symptom, adding materially to the inconvenience and suffering of the patient; in such cases, although aperients are indicated, and of the greatest utility when properly selected, they frequently add to the disease and suffering already present if violent and not combined with sedatives. The best aperients that can be used in these diseases are combinations of the pill. hydrargyri with rhubarb or aloes, combined with the pil. galbani co., the extracts of hops, lettuce, or hyoscyamus, or the salts of morphia. Calomel, combined with the pil. aloes comp. and some sedative, is also in certain cases useful. The proportion of the mercurial for each dose should rarely exceed one grain. These remedies, with solutions of the neutral salts in bitter infusions, to which the hydrocyanic acid is added, are the forms of aperient which I have invariably found most useful; they operate freely without pain or uneasiness, and generally afford the patient very marked relief.

“*Sedatives*.—This is an important class of remedies in diseased conditions of the mucous membrane of the stomach. In all forms of inflammation, there is mostly an exalted state of the sensibility of the part inflamed. The peculiar organization, however, of certain nerves, particularly those of the ganglionic system, and the system of the par vagum, render the exalted sensibilities of the mucous surfaces of

the stomach and intestines inappreciable by the brain, unless they pass a certain limit. Hence, in some instances, inflammatory disease of these organs proceeds to actual disorganization, without the patient being aware of its existence; whilst, in others, a slight degree of inflammation will produce intense febrile excitement. It is from a knowledge of the peculiar sensibilities of these parts that we may see the great use of sedatives in the treatment of their inflammatory or other forms of disease; and it is, also, from this circumstance, that I never prescribe an aperient remedy in diseased conditions of the stomach without combining it with some preparation of morphia, the hydrocyanic acid, or the extract of henbane. The best sedative that can be employed is the muriate of morphia. In all inflammatory affections of the stomach, this remedy, combined with aperients or with alkalies, or given merely to allay pain or irritability, is of great use. Others, however, may be given according to circumstances, such as the acetate of morphia, the hydrocyanic acid, the liq. opii sedativus, or the extracts of hop, lettuce, or henbane. These are the chief sedatives in use in such affections, and perhaps they answer all the necessary indications. They are generally more useful in combination with alkalies or aperients than when given simply and uncombined. The external application of sedative remedies to the epigastric region, in many painful affections of the stomach, I have found of very great service, whether these affections are primitive, or whether they result from organic change. In cases of extreme thickening of the coats of the stomach, of a scirrroid character, where the suffering patients are almost worn out with constant pain, such remedies, applied over the epigastrium, most materially alleviate the distress. We have no hope, in many of these deplorable cases, of eradicating or curing the disease; but still we may afford great ease to the patient, enable him to follow some gentle occupation, and to digest a mild unirritating food without pain, and even with comfort. In many forms of disease, a piece of flannel soaked in a strong solution of opium, and worn over the epigastrium, affords great relief: or, what is more efficacious, a solution of from two to four drachms of the extract of belladonna in six or eight ounces of water, to be used tepid as an application to the part. A poultice of the leaves may be also applied, or one of hyoscyamus, hop, or poppy. These remedies are highly serviceable in all diseases of the stomach accompanied by pain. They may be employed with equal service in acute inflammatory affections of this organ, or where gastric irritation occurs as a complication in fevers and inflammatory diseases generally.

“*Antacids and Absorbents.*—We learn from the researches of Prout, Tiedemann, and Gmelin, and Dr. Beaumont, that the gastric juice, during digestion, contains free hydro-chloric and acetic acids, and that these acids are furnished in greater quantity in direct proportion to the more or less stimulating qualities of the food. Under a mild farinaceous diet these acids are barely detected. Many diseased conditions of the stomach are accompanied by this increased generation of acid in its secretions, and in some instances intense

acidity after food forms one of the most prominent and distressing features of the complaint. Great intestinal irritability also frequently accompanies this disposition to the formation of acid in the stomach, and the patient is commonly tormented with tenesmus, or purging of small, watery, hot, stools. In these cases it becomes necessary to administer remedies to neutralize this excess of acid, as well as to adopt others to prevent the disposition to its recurrence. The *mistura cretæ* of the *London Pharmacopœia*, given with large doses of hydrocyanic acid, is here particularly serviceable, more especially in those forms in which intestinal irritation is present. I have found this combination of remedies of singular service in gastric diseases attended with a profuse secretion, or formation of acid. At other times the ponderous carbonate of magnesia, or soda, combined with morphia and the sub-nitrate of bismuth, may be employed.

“It occasionally happens, that intense acidity accompanies or succeeds to a state of great bodily weakness, particularly when this is the result of large losses of blood, as in the cases I have detailed in the section of the work on diseases of the stomach produced by anemia. Here the condition of the stomach is dependent upon that of the constitution generally, and yields to remedies suited to the general affection upon which the morbid state of the stomach appears to depend. In these forms of disease, the various preparations of iron are eminently serviceable: combinations of the carbonate of iron with myrrh and rhubarb, are perhaps amongst the most efficacious that can be employed.

“*Tonics*.—There are many forms of gastric disturbance which resemble hyperemia, or inflammation, which are not benefited, but rather rendered worse, by a treatment framed to suit such forms of disease; hence it is, that we very commonly see local depletion from the epigastrium and aperient medicines injurious to many affections of the stomach, which we should have supposed, from the symptoms which they exhibited, would have been benefited by such remedies. It is because these symptoms depend upon some other cause, and not upon hyperemia or inflammation, that these remedies are not successful; and yet it is difficult, nay, sometimes impossible, to distinguish between vascular irritations of the stomach and other affections of this organ, the results of treatment being occasionally our only guides.

“It is in many of these states, that we find tonics succeeding where an antiphlogistic treatment has failed, or been positively hurtful. I subjoin one or two cases, by way of example.

“A gentleman consulted me, complaining of unpleasant symptoms connected with his head and stomach, which invariably harassed him after eating, and during the process of digestion. He had swelling of the stomach, nausea, flatulence, uneasiness and tenderness in the epigastrium, with great drowsiness and stupor. He took, at first, combination of the blue pill with rhubarb, and the stomachic aperient mixture prescribed at page 284. These remedies afforded no relief; the patient was worse after their use, and yet the symptoms characterizing his complaint, both in the head and stomach,

resembled very much a condition of hyperemia in both organs, which we see so frequently benefited by such a plan of treatment. I now recommended the carbonate of iron, to be taken in combination with myrrh and rhubarb. The use of these remedies, for a few days, completely removed the affection of both organs. This is not a case of rare occurrence; I have repeatedly noted cases of pain in the stomach, with constipation, heat, beatings, and tenderness in the epigastrium, exactly resembling hyperemia of the stomach, or the nervous centres of the epigastrium, aggravated by leeches to this region, and the use of aperients, yield readily to such remedies as those I have just noticed.

“ I have repeatedly been consulted by a young lady, who suffered constant pain in the stomach after eating; at the same time her bowels were habitually constipated, the epigastric region was hot and tender, and the seat of strong pulsations; the tongue dry and coated, the papillæ vivid and largely developed. No permanent relief was afforded in this case from local bleeding or aperients; after the complaint had been established for some time they were injurious. Great benefit was derived subsequently from the carbonate of iron, with myrrh and rhubarb.

“ Tonics are useful in many morbid states of the stomach, which may be referred to four classes:—

“ 1. Primitive morbid conditions, resembling inflammatory affections, which are aggravated by an antiphlogistic treatment, or by aperients.

“ 2. States of disease, succeeding to inflammation, which have been benefited by an antiphlogistic treatment in the commencement, but where this no longer affords relief, or adds to the severity of the symptoms.

“ 3. Various morbid states of the sensibility of the stomach. These are occasionally accompanied by intermittent neuralgic affections in other parts of the body.

“ 4. States of general debility, and many local symptoms, as pain, nausea, and vomiting, which accompany confirmed organic diseases of the stomach.

“ I have shown in the former parts of this work, that there are certain forms of irritation in the stomach which succeed to affections, in the first instance inflammatory, which are benefited by a tonic treatment. The disease, at first one of inflammation, having been subdued, terminates in one of debility; and the remedies found useful in the earlier forms of the affection, become decidedly injurious in the latter. We often find pain, distention, acid eructations, nausea, and vomiting, with tenderness in the epigastrium, succeed to irritations which have been, at the onset, benefited by leeches to the epigastrium, warm aperients, and a rigid diet. These symptoms are very liable to come on as the sequel to inflammatory affections of the stomach which have been treated with large losses of blood from the epigastric region. Most of the cases detailed by M. Barras, cured by tonics and a full diet, were diseases of debility in the stomach produced in this way. The great art in managing affections

of the stomach of this kind is to mark the point at which the disease ceases to be inflammatory, and passes into one of debility or of sensibility. It is at such a time that the antiphlogistic plan should be abandoned, and tonics and a fuller diet substituted for it. I have frequently been consulted by patients who have been reputed the subjects of confirmed chronic gastritis; they have been pale, emaciated, tormented with pain and sickness, and have had stomachs so sensible that they could hardly take the least particle of food without pain. These persons have been treated with leeches to the stomach, which have at first afforded relief; subsequently, by different modes of counter-irritation, blisters, and the tartar emetic ointment; still they go on getting worse, the gastric symptoms continue, and they are supposed to be suffering from an incurable chronic gastritis. These cases were originally inflammatory affections; the inflammation has been subdued, and passed on to a disease of debility. This has been accompanied by its peculiar symptoms, which have been supposed to depend on a continuance of the inflammation, when they were due to an opposite cause. By placing such cases on a full diet, and exhibiting freely the carbonate of iron, the symptoms have soon given way, and the patients have speedily recovered their usual health, all the unpleasant symptoms connected with the stomach speedily subsiding. The combinations of the carbonate of iron with the muriate of morphia, in such cases, if much pain exist, or with rhubarb, if constipation be present, are perhaps among the best remedies that can be employed."

Philadelphia Medical Examiner.

WE have received the first five numbers of this excellent weekly paper, and have been much pleased and instructed by their contents. We extract from a lecture by *Dr. Coates* the following remarks on the *Lobelia inflata*:

"At the trial of Abraham Thompson, it was deposed that he was in the habit of making use of often repeated emetic doses of the lobelia inflata. I may remark that Dr. J. R. Coxe states that he has seen death result from the use of the lobelia, without its producing vomiting. The Thompsonian course consists in a tea spoonful of the lobelia every fifteen minutes, till it produces vomiting. This is followed by the steam bath, and afterwards by the administration of what Thompson is stated to have originally called his *coffee*; but which is now styled "composition tea." This, I have been informed, is a strong decoction of a mixture of red pepper, mustard, cloves, and the candleberry myrtle; or, as they call it, the bay-berry root bark.

"At the recent trial at New York, it was deposed that the bay-berry root bark is a poison, but it was alleged that the Thompsonians administer large quantities of it with impunity. The popular designation of this plant (bay-berry) is, I think, very incorrect. The term

bay is universally applied to the laurel. One of our earliest authorities for this, and there can be no better, is the English version of the Scriptures. I would therefore reject this name bay-berry, which is a mere local usage of a part of New England. I exhibit to you, gentlemen, the bark of the stem of this plant: the bark of the root I could not obtain, without applying to sources to which it would be unpleasant to address myself. The Shakers have usually the best assortment of our native medicinal plants to be met with. The bay-berry is described as being very acrid, even astringent in its taste; these qualities you can now test by personal observation.

“I administered, in my recent trials, the powdered lobelia in the dose of about thirty grains, and with very great benefit, to the man Rogers, who is labouring under severe asthma, with dilatation of the bronchiæ. The tincture of lobelia has long been administered, as you know, in cases of dry asthma. A patient formerly in the hospital has been relieved by the use of it for a series of winters. The dose of it has been from ten to forty drops, three times a day; and, sometimes, it may be pushed to larger doses. The Rev. Dr. Cutler, of Massachusetts, first called our attention to this remedy. Dr. Bigelow, of Boston, states that he has used it in table spoonful doses. I cannot well reconcile this discrepancy; and this circumstance strongly points out the necessity of extending our observations. The tincture is made from the fresh plant. When I was apothecary in this house, I remarked that some tinctures of recent vegetables were made much weaker from being diluted with the watery juices of the plant. Dr. W. P. C. Barton states in his Botany, that the tincture of the fresh lobelia is stronger than that from the dry. This is contrary to the remark which I have just made.

“The lobelia in over-doses causes dizziness, nausea, and the other usual effects of so many narcotics. Dr. Smith (the resident physician) and myself watched the effects that were produced in the man, Rogers, from immediately after the administration of the remedy. The dose was one tea spoonful; and a similar spoonful weighed twenty-nine grains. In four minutes after he had taken it, active vomiting was produced, which, at the end of twelve minutes, was entirely over. The matter vomited was water tinged green from the colouring matter of the lobelia. No headach nor dizziness occurred; the only result was a diminution of the cough, and of the dyspnœa and anxiety. The absence of the bad symptoms is probably owing to the short time which the lobelia remained in the stomach. In order for a narcotic to produce its poisonous effects, it must of course be retained. Orfila, you know, in making his experiments with poisons, invariably tried the œsophagus, to prevent vomiting. Taking this into consideration, we may be struck with a nearer view of the resemblance of these to ipecacuanha, and other more familiar emetics. These also possess the power of producing nausea, salivation, dizziness, weakness, coldness, sweat, and other symptoms commonly attributed to poisoning, and if long retained and in considerable quantity, these phenomena may become alarming.

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PART I.

ORIGINAL COMMUNICATIONS.

ART. XIV.—*On the State of the Pupil in Typhus, and the Use of Belladonna in certain Cases of Fever.* BY ROBERT J. GRAVES, M.D.

THERE are two classes of narcotic remedies, which are known to possess alike the power of allaying nervous excitement, abating pain, and producing sleep, but whose effects on the state of the eye are very different, one class causing contraction, the other dilatation of the pupil. Among the former, opium holds the highest rank, among the latter, stramonium and belladonna.

In the state of sound healthy sleep, the condition of the pupil is remarkable, and worthy of the attention of the physiological inquirer; it is very much contracted, a fact repeatedly demonstrated, and of which any one may convince himself, by cautiously opening the eyelids of a person fast asleep. During the hours of repose, when all the organs, except those more immediately connected with the continuance of

life, suspend their functions and enjoy a restorative calm, vision being no longer necessary, Nature interposes a curtain, to prevent the entrance of light, and protect the delicate tissues of the eye from an influence at once useless and injurious. But the moment we awaken, and the mind requires the use of the senses, the aperture for the admission of light instantly resumes its ordinary dimensions.

This, however, is not the case when the system is under the influence of narcotics. When contracted by the use of opium, the pupil does not dilate to its usual size after the patient awakes, but continues more or less contracted, according to the mode in which the drug has been administered. Thus, in the case of a lady who had swallowed more than half an ounce of laudanum through mistake, Mr. Swift of Kingstown found that the pupil did not resume its ordinary dimensions until the third day after the accident, although the constitutional symptoms of narcotism lasted but for nine or ten hours. Where the opium has been exhibited in small doses, the pupil will have a tendency to remain contracted to a smaller size than natural, and though it dilates a little in a weak light, its aperture will be narrower than usual when the light becomes clearer.

Something analogous occurs with respect to those narcotics which have a tendency to produce dilatation of the pupil. When used in small doses, they first render the pupil somewhat larger and more sluggish than in the normal condition, in which state it continues until the effects of the narcotic pass away; but if the latter be continued, and in augmented doses, the pupil becomes permanently dilated, and insensible to the influence of light.

Here, then, we have two classes of remedies which appear to exert a very remarkable effect on the animal economy, possessing alike the power of allaying nervous excitement, relieving pain, and producing sleep, but which are found to have quite opposite effects on the pupil. We administer a few doses of

stramonium or belladonna to a patient labouring under great nervous excitement and sleeplessness, and find that it tranquillizes the one and diminishes the other. We prescribe opium in a case of fever accompanied by total loss of rest, and extreme nervous irritation, and its administration is followed by an equally favourable result. But in the one case our remedy produces dilatation of the pupil, in the other contraction. Are we to employ these remedies indiscriminately? Is their use a matter of indifference, or should we, in the treatment of a certain case, choose one in preference to the other? These are difficult questions, but of great importance, and well worthy of attentive investigation.

Let me observe here, that there are unfortunately too many subjects of deep interest in a practical point of view, on which our knowledge is still very indeterminate, and which remain to be settled by the results of future experiments; indeed many of our medical doctrines, involving considerations of great, nay vital moment, are still in an uncertain and fluctuating position. The matter to which I have just now adverted, is one of this class; its importance is obvious, and yet it has never been made the subject of observation or inquiry. Another practical question equally undetermined is, in the treatment of certain forms of disease, when should we select acids, and when alkalies? One would imagine that this must have been long ago settled, and that every tyro ought to know when the alkalies should be administered, and when acids are required; yet it is a question by no means decided. To prove the truth of this assertion, it is only necessary to refer to the works of Bateman and other writers on diseases of the skin, all of whom recommend acids and alkalies in many cutaneous affections; but they do this without any apparent or fixed principle, capable of enabling us to determine beforehand which of the two is more likely to prove beneficial; and consequently, in actual practice, it is not unusual to find nitric acid prescribed, where soda had failed, and *vice versa*.

In some forms of pseudo-syphilis, and in debilitated cachectic states of the system, we are accustomed to prescribe sarsaparilla and nitric acid, or sarsaparilla and lime water, liquor potassæ, carbonate of soda or potash, and other similar remedies. The same thing may be said of chronic cough, in which I have seen much benefit from sarsaparilla with nitric acid, and from sarsaparilla with carbonate of soda or lime water. I may also observe, that I have cured some cases of dyspepsia with acids, after having failed with alkalies, and have, in this disease, advantageously employed alkalies where acids had been tried in vain. The matter is indeed still quite undetermined, and much remains to be done by the practical physician before we can arrive at any fixed laws to guide us in our selection of acid and alkaline remedies, or inform us in what particular cases either should be preferably employed.

It appears certain, that the state of the brain, attended with contraction of the pupil, must be very different from that which is accompanied by dilatation. In some diseases of the brain, we find the pupils of very small dimensions, in others dilated, and almost wholly insensible to the stimulus of light. It is obvious that these two opposite conditions of the pupil cannot depend on the same condition of the brain. If it be asked, on what particular conditions of that organ do they depend, the answer is, we cannot tell. Pathologists have indeed written and reasoned a great deal on this subject, but they have discovered nothing; they have found nothing on *post mortem* examination sufficient to explain, in a satisfactory manner, why, in one case of fever with cerebral affection, we have contraction, in another, dilatation of the pupil. A fever patient dies with suffusion of the eyes, sleeplessness, and *contracted pupil*; and on dissection you find vascular congestion, sub-arachnoid effusion, and other marks of inflammatory action, quite sufficient, in the opinion of many, to account for the phenomena in question; but on the other hand, you meet with another patient in exactly the same

state, and on examination after death, you are not able to detect any marks of determination to the head, much less of inflammation. Again, we have a patient in the latter stage of fever with numerous symptoms of cerebral disturbance, accompanied by remarkable *dilatation of the pupil*; but we cannot point out after death any thing in the state of the brain, different from that observed in the case with contracted pupils, and consequently capable of explaining that dilatation. We are, and I fear must long remain ignorant of the peculiar, and probably very dissimilar conditions of the brain on which these opposite states of the pupil depend, and all we can expect to arrive at, in fact all we should study to investigate is, what class of remedies is most appropriate for each.

In fever with cerebral disease, one of the most alarming symptoms is marked contraction of the pupil, and were I called to a case in which every other symptom was favourable, but great contraction of the pupil present, I would say that it was a case of extreme danger. A tendency to even moderate contraction of the pupil is a very dangerous symptom in typhus, but a pupil extremely and permanently contracted, or as it has been called a *pinhole pupil*, is, or used to be, a fatal sign. Although this symptom is so obvious, so easily ascertained, and unhappily so common, for I have within the last few years witnessed it in a great number of fatal cases, yet, strange to say, it has not attracted the attention it deserves, among writers on typhus. No particular notice is taken of it by any author on Irish fever that I have seen. *By some it may be mentioned incidentally, but by none is the state of the pupil treated of specially, and in detail.* In Barker and Cheyne's valuable work, which contains contributions from every man of eminence of the day in Ireland, this omission is observable, and the same may be said of all the very valuable Reports from the Cork-street Fever Hospital. I have looked over many volumes of cases taken by myself, when physician to the Whitworth

Fever Hospital, Drumcondra, as well as my Report on the Epidemic of 1822 in Galway, and in none of these documents is this question touched on. I and others have indeed noted cases where both the pupils were dilated, or one was larger than the other, but we seem to have neglected their tendency to contraction, and their actual extreme contraction; I do not mean to assert that contraction of the pupils has not been described as occurring in various cases, but I do say that its importance and meaning have not been as well appreciated in fever as in apoplexy. In the latter disease physicians have long since perceived the great danger portended by this symptom, but in fever they have not taken proper pains to compare the different states of the pupil in different cases, and consequently *they have altogether neglected this symptom as an important indication or contra-indication to the use of certain narcotics.*

Whenever, in attending a case of fever, you meet with this contracted state of pupil, even in a slight degree, although your patient may be restless and greatly in want of sleep, beware of opium. I have often seen it tried, and I think scarcely ever without more or less injury to the patient. When opium is administered to a patient in the advanced stage of fever, with symptoms of cerebral derangement, and tendency to contraction of the pupil, you will find that the pupil which has been moderately contracted to-day, will be greatly contracted to-morrow, and that the patient will soon sink into an irrecoverable state of coma. This contracted state of the pupil may exist in its extreme and most marked form in typhous fever, without being necessarily accompanied by headach and delirium; the patients are restless, sleepless, and in a state of remarkable nervous excitement; but they answer questions not unfrequently in a tolerably clear and rational manner, [and many of them distinctly affirm that they have no pain in the head. These circumstances may deceive the unwary, but the experienced practitioner, who has witnessed many such cases,

will feel that a fatal termination is threatened. Under these circumstances, opium in every shape is injurious, and even tartar emetic fails in controlling or diminishing the pernicious effects of opium. This is somewhat curious, as the combination of tartar emetic and opium seldom fails in relieving cases similar in all respects, except the symptom of contraction of the pupil. Some time ago I attended, with Surgeon White and Doctor Corrigan, a gentleman of sound constitution labouring under typhus. During the course of his fever, his head became greatly engaged, and he had remarkable contraction of the pupil. We prescribed tartar emetic and opium without effect, and we had the chagrin of seeing a man of vigorous frame, and in the prime of life, carried off in spite of all our efforts. I was grieved at the result of the case, and talked over the matter very fully with Dr. Corrigan and Mr. White. During the course of our conversation, Dr. Corrigan threw out the suggestion, that in such cases narcotics which produce dilatation of the pupil might be beneficial. The observation struck me as being curious and important, and I determined to try it on the first favourable opportunity.

It may appear somewhat extraordinary to think of prescribing for a single symptom, and giving belladonna in a case of fever with contraction of the pupil, merely because it produces dilatation of the pupil ; but, as was before observed, it is not unreasonable to suppose that the state of the brain which accompanies dilatation of the pupil is different from that which accompanies contraction, and if belladonna has an effect in producing that cerebral state which is attended with dilatation, it is not going too far to infer, that its administration may do much towards counteracting the opposite condition ; neither is it unphysiological to conclude, that if a remedy be capable of counteracting or preventing *one very remarkable effect* of a certain morbid state of the brain, it may also counteract *other symptoms* connected with the same condition. Thence it was that I was induced to try belladonna in the advanced stage of

fever, accompanied by marked cerebral irritation, and a contracted state of the pupil. Before I became acquainted with this plan of treatment, I always looked upon cases of this description as hopeless, for with the exception of one man, I had never previously seen a fever patient recover, who had exhibited marked contraction of the pupil ; while, strange to say, I have seen many get well after the pupils had become very dilated. During the course of last Spring, one man, who had marked contraction of the pupils, recovered under the use of spirit of turpentine. The success of this case induced me to think, that in spirit of turpentine I had a valuable remedy in the treatment of cerebral disease, with contraction of the pupil, but after several trials I found that I was mistaken.

I am anxious here to guard myself from being misunderstood, or intending to express the opinion that dilatation and contraction of the pupil in typhus invariably indicates states of the brain constantly the same, and always attended by the same groups of symptoms, one peculiar to cases with contracted, the other only found in cases with dilated pupils. Such an opinion would not agree with facts, for I have seen the pin-hole pupil in the violent delirium of fever, when the senses and imagination are morbidly active, and also in the opposite state of coma and insensibility. Extreme dilatation of the pupil is a much rarer phenomenon in typhus, and seldom makes its appearance before the last few hours of life. I do not think that I have ever observed the pupils to be greatly and permanently dilated during the state of delirium and hypersensibility, but I have often seen them somewhat enlarged and sluggish under such circumstances. But that, as a general principle, violent delirium and excitement are not incompatible with extreme and permanent dilatation of the pupil, appears evident from Dr. Peddie's cases of poisoning by a certain species of mushroom, as detailed in the *Edinburgh Medical and Surgical Journal* for January, 1838. On these cases Dr. Peddie makes the following observations. "The only other remark I shall offer on the phenomena

observed in cases number one and two, is on the curious changes in the condition of the pupil. In the elder Mr. Bain's case, *during the stage of perfect insensibility*, the pupils *were very much contracted*, and were unaffected by light, but when the system was roused to the fury of delirium, they became both *widely dilated*, and *were, to a considerable degree, sensible*. In the boy M'Leod's case, it is unfortunate that the state of the pupil could not be ascertained during his sopor. When seen first, however, and while he was in the height of delirium, *they were widely dilated, but quite unaffected by light*, and only increased in contractility as the delirium passed away. Another singular phenomenon of M'Leod's pupils, was that which was observed at half past six P. M. (At that hour he was in a sound natural sleep, into which he had fallen a few minutes before. The pupils were contracted, but a little further contractile by light. On being roused, however, they became widely dilated.) Such changes as the last, however, I have of late very frequently observed in the course of different affections of the head, particularly in those of children. When asleep, or lying quiet, the functions of the brain being undisturbed, the pupils are often contracted to a very small size; but when the patient is roused by any sort of irritation, more especially by forcible pressure on the epigastrium, the pupils may be suddenly dilated to their full extent, and often, while in this state, refuse to contract to the most powerful stimulus of light. This curious vacillation of the pupils, and indeed their very variable phenomena in different states of disease, only shews, that symptoms derived from the state of the pupils cannot, in the present state of our knowledge, be much depended upon as precise and invariable indications of particular cerebral conditions."

These cases of Dr. Peddie's are of extreme interest, and prove that an opinion too generally entertained, that dilatation of the pupil only attends the state of coma or insensibility, is not correct. In a paper long since published in this Journal, I have noticed the many facts connected with the conditions of the pupil

observed in hydrocephalus and fever, which facts, I think, are not reconcileable with the commonly received explanation of the cause of dilatation of the pupil. In truth I am persuaded, that contraction and dilatation depend not on mere increase or diminution of pressure on the brain, but on some inscrutable states of the vital functions of that organ. But though we may be unable to ascertain their causes, yet the study of these phenomena, and of the circumstances under which they occur, cannot fail to elicit useful results, particularly when compared and examined for the purpose of ascertaining how far they may be rendered serviceable in guiding us to a more useful treatment of cerebral symptoms. Hitherto, in the treatment of fever, physicians have been too much in the habit of considering cerebral symptoms as the result of congestion or inflammation, or as arising from a derangement of the balance between the venous and arterial circulation. Now I am of opinion, that many of these symptoms arise from causes altogether different, and which, in the effects produced, bear a close resemblance to poisons. In fact, no tissue, no secretion, no solid, no fluid in the body continues healthy in composition and texture during the entire course of a severe fever, and consequently the functions of every part, the brain among the rest, must be more or less deranged. The changes all the organs undergo may be now and then modified by the accidental supervention of inflammation, but the latter is only accidental, and this must be steadily held in view, in entering upon the consideration of treatment. And again, as we do not understand in what the alterations of function and nutrition consist, it follows necessarily that we must decide upon the value of our remedial agents more from observation and experiment than from any preconceived opinions.

I shall now proceed to make a few remarks with respect to the administration of belladonna in cases of cerebral excitement in fever, accompanied by contraction of the pupil; of stramonium I shall say nothing, having as yet no experience of its ef-

fects. I have used the belladonna by itself, or combined with musk, or with opium, or opium and tartar emetic. It is of course of the greatest consequence that the extract of belladonna employed should be extremely good. Its efficacy can be determined, and that in a very short time, (a few minutes at most,) by applying a little, mixed with a sufficient quantity of water, to render it semi-fluid to the eye of a healthy person. If it produces immediate dilatation of the pupil it can be relied on.

Belladonna is a powerful remedy, but has not, in this country, been used much internally ; in Germany it is very often prescribed in neuralgic and spasmodic diseases, and has proved very useful in hooping-cough ; of late it has been recommended as a prophylactic against scarlatina, and certainly the experience of my friend Dr. Oppenheim, when attached to the medical staff of the Turkish army, appears very strong in favour of the supposition, that small doses of this extract, frequently repeated, render the generality of constitutions proof against the contagion of scarlet fever. Its property of dilating the pupil, discovered by professor Himmly of Göttingen, has proved of the greatest advantage to ophthalmic surgeons. When applied in the form of plaster, as it very frequently is in Dublin, for the purpose of relieving local pains, it often produces its peculiar effects on the eye, even though applied to a distant part of the body, and the patient complains of indistinct, or even double vision.

Stramonium is also a very powerful narcotic, and like belladonna, must be carefully watched when given internally. The dose of each should, in fever, be small, but may be repeated until the desired effect is produced. Of course belladonna cannot be expected to succeed in all cases, with contraction of the pupils, for in several, the coexistence of other symptoms precludes the hope of recovery.

Batley's sedative, the black drop, morphia with its various salts, and in short all known preparations of opium, seem to ex-

ercise the same influence on the pupil as opium itself, and consequently are all equally objectionable in those cases where the pupil is contracted, or evinces a tendency to become so. Whether this tendency can be neutralized by combining belladonna or stramonium with them, forms a most interesting subject for clinical experiment. As far as I can judge from the result of the cases in which I have tried this combination, it promises to be a most useful addition to the remedies usually employed in fever. This combination evidently saved the life of a gentleman in parliament-street, whom I attended along with Mr. Sibthorp and Dr. Dwyer, and whose case was apparently hopeless. In the Meath Hospital, draughts consisting of black drop, belladonna, and tartar emetic, have been eminently serviceable; the extract of belladonna which I have used, is, properly speaking, an inspissated juice, and is so called in the last edition of the Dublin Pharmacopœia.

I shall now proceed to relate some cases in which I employed belladonna with benefit. In addition to many treated at the Meath Hospital, the following occurred; the first was noted by Mr. Moore of Mount Pleasant, who attended the patient from the commencement, and to whose diligence and assiduity I can bear ample testimony.

“A married lady, rather advanced in years, residing in the suburbs of Dublin, was attacked with rigors on the 30th of June, 1837, having exposed herself to fatigue on the preceding day. She went to bed very ill, complaining of headach, thirst, loss of appetite, pains in the loins, and total prostration of strength. She continued feverish up to the 6th of July, but without any material change in her symptoms. During this period she was treated with mild aperients, blue pill and James’s powder, and effervescing draughts. On the 7th the headach increasing, Mr. Moore applied eight leeches to her temples, and ordered her head to be shaved, which, however, she would not permit. She appeared better at night after the application of the leeches, and thought her headach was much

relieved, but next morning she was much worse, her breathing laboured, her pulse small and quick; her tongue brown and parched, and her countenance remarkably anxious. She had frequent tremors, particularly of the right side, was agitated and sleepless, unconscious of what was passing round her, and on examination her pupils were found contracted to an extremely small diameter. Mr. Moore applied a blister to the nape of the neck, but finding no improvement in her symptoms requested Dr. Graves to visit her in the evening. We directed her head to be shaved, and the scalp covered with napkins steeped in vegeto water; chicken broth and a little beer were allowed occasionally, and the following pills were prescribed, one to be taken every third hour.

Rx Extracti Belladonnæ, gr. i.
—— Hyosciami, gr. vi.
Pilulæ Hydrargyri, Θ i. M.
Fiant Pilulæ vi.

“After having taken the third pill, she became more quiet, and the tremors were not so violent; and after the fourth, she fell asleep, awaking at intervals during the night. Next morning she was more composed, and her pupils more dilated; her pulse was still very weak and small. As she would not take the beer, I gave her wine under Dr. Graves’s directions, regulating the quantity so as not to increase the heat and pain of the head; she also got chicken broth frequently. The pills were continued, and her bowels were kept open by enemata of infusion of senna, Epsom salts, and tincture of jalap. On the 10th she was better, but as her breathing was rather oppressed, and some epigastric tenderness present, I applied a blister over the stomach. As she now slept well, and the pupil had resumed its natural condition, the belladonna pills were given less frequently. This treatment was continued from the fifteenth to the twenty-first, during which period she took one or two pills every day, and used a little brandy and water instead of wine. During this time she could not recognize any one, her pulse

was slow and feeble, occasionally intermitting, and her hips were beginning to strip ; she also had occasional fits of shivering, which were invariably relieved by taking the pills. On the 21st I was enabled to discontinue the cold to the head, and administered a turpentine enema. She now became much better, the tongue cleaner, the pulse stronger and more steady, and she began to exhibit every sign of returning consciousness. The enema was continued every day, or other day, as occasion required. On the 26th she was able to sit up in bed, and is now quite recovered."

Another case treated in the same way is still more remarkable. The patient was under the care of Dr. O'Reilly of Dominick-street, who has kindly furnished me with accurate notes ; the following is his report:—" Mr. B., aged thirty, of full plethoric habit, but nervous melancholic temperament, and who had lost a brother a few months before by fever, was attacked on the sixth of September with the usual symptoms of typhus. I saw him on the ninth, and found his symptoms were those of ordinary fever, accompanied by pain in the fore-part of the head, for which he was ordered leeches, cold affusion, aperients, and effervescing draughts. He continued without any change, except relief of his headach, until the eleventh, when his skin became maculated. On the twelfth, when first seen by Dr. Graves, his skin was covered all over with dark-coloured maculæ, his countenance anxious, tongue white, pulse 130, small and compressible, great restlessness and want of sleep, but no evidence of determination to any of the more important organs. On the evening of the fourteenth he was attacked with diarrhœa, followed by a reduction in the temperature of the skin, and great prostration of strength. As he had slept none since my last visit, I ordered him to take a teaspoonful of the following mixture, to be repeated every second hour until rest was procured.

Rx Acetatis Morphicæ, gr. i.

Syrupi Pap. Alb. ʒ ii.

Misturæ Camphoræ, ʒ ss. M.

“ 15th. Diarrhœa has ceased ; some sleep after taking a few doses of his mixture ; febrile symptoms as before.

“ 16th. Fulness of the abdomen ; no stool for the last twenty-four hours ; countenance of a dull red hue ; eyes suffused ; respirations forty in a minute ; pulse extremely quick ; has passed no urine for the last twelve hours ; decubitus on the back ; slight delirium. A considerable quantity of urine was drawn off by the catheter with relief, and he was ordered an aperient draught of castor-oil and tincture of rhubarb. During the course of the evening his delirium increased, and the pulse became intermittent ; his skin was cool, and he appeared greatly prostrated. Two glasses of claret to be given during the night ; ice to the scalp ; and his anodyne to be repeated if necessary.

“ 17th, (eleventh day of fever.) An exacerbation of all his symptoms, with contraction of the pupils. Dr. Graves saw him with me, and recommended the following draughts, one to be taken every sixth hour ; also to increase the quantity of claret to a pint in the day.

Rx Moschi Veri, gr. x.

Extracti Belladonnæ, gr. i.

Mucilaginis Acaciæ, ʒij.

Misturæ Camphoræ, ʒss.

Syrupi Aurantii, ʒiss. M. fiat haustus.

18th. Has passed a better night ; pupils more dilated ; countenance improved. Respiration and pulse as before.

Contin^r. vinum et medicamenta. Beef tea.

“ 19th. Slight diarrhœa, in other respects improved.

“ 20th. The diarrhœa continuing, we ordered chalk mixture, Port wine, and coffee ; his draughts to be continued every sixth hour as before.

“ 21st. Diarrhœa has nearly ceased ; pulse less frequent ; tongue cleaner ; subsultus and delirium, which have been extreme during the last seven days, considerably diminished. The patient has also had some comfortable sleep last night.

“ 22nd. Has slept much since our last visit, and is more intelligent.

“ 23rd. Much improved ; bowels not freed since yesterday.

Haustus Rhei cum Magnesia.

“ From this day until perfect convalescence nothing occurred worthy of notice.”

Besides the foregoing cases I have witnessed others in private practice, and in the Meath Hospital, in which belladonna seemed to produce very favourable effects. I do not wish to speak more decidedly on the matter at present, as much remains to be ascertained by future experiments. Enough, however, has been done to stimulate inquiry, and it will be granted by all, that there is no subject more deserving of the earnest attention of the physician than the treatment of cerebral affections in typhus fever. Where a tendency to contraction of the pupils accompanies this disease, the usual plan of blistering, leeching, cold lotions, &c., will prove insufficient. It may be of some use in the commencement, or in the middle stage of fever, but not towards the end, when the vital powers are excessively reduced, and the patient hangs on the verge of dissolution. On a future occasion I shall give the details of a very remarkable and dangerous case of fever, in which the belladonna was given in repeated doses during thirty-six hours, at the end of which time it overcame the cerebral excitement, and the patient recovered.*

* In considering the causes which produce dilatation of the pupil, I omitted to mention venesection ; as syncope approaches, we observe “ paleness and coldness of the skin, and shrinking of the features ; cold drops of sweat hang on the forehead ; the eyes look glassy, and the pupils are dilated.”—See CLUTTERBUCK’S *Lectures on Bloodletting, in the Medical Gazette.*

ART. XV.—*Statistical Report of the Richmond Lunatic Asylum.* By JOHN MOLLAN, M. D., Physician Extraordinary to the Asylum ; and one of the Physicians of the Whitworth Hospital, Drumcondra.

[Read at the Evening Meeting of the College of Physicians at the College Hall, March 26th, 1838.]

BEFORE the erection of the Richmond Lunatic Asylum, which was completed in the year 1814, the only receptacle for pauper lunatics in this city, with the exception of a portion of Swift's Hospital, was the House of Industry ; but the part of this establishment at that time appropriated to the insane was so limited in extent, and so defective in arrangement, and the patients were so numerous, and so much crowded together, as to preclude the adoption of any systematic plan for their moral or medical treatment. A representation of these circumstances having been made to the Government by my respected colleague Doctor Jackson, who has ever been the strenuous advocate for the kind treatment of the insane, and for attention to their comforts ; the formation of an asylum was determined on, unconnected with the House of Industry. Before the commencement of the building, much care was bestowed on acquiring information as to the best arrangements of such establishments ; and it was planned not simply for the safe-keeping of the insane, but with a view to their rational treatment and cure, and it was the first institution of the kind in this country constructed for the classification of the patients according to their several states of disease.

For many years after its opening, patients were admitted into the Richmond Asylum from all parts of Ireland ; but since the year 1830, when the various District Asylums were completed, it has been appropriated to a certain district, which embraces the city and county of Dublin, the counties of Meath, Louth, and Wicklow, and the town of Drogheda. A residence of

twelve months within the district is necessary to entitle a patient to be received, and none are admitted without a medical certificate of insanity, and an affidavit on the part of the nearest relative, that the individual is a pauper, by which is understood, not being possessed of sufficient means to pay for maintenance in a private asylum. A few persons have been occasionally admitted, who contributed small sums to the funds of the institution; but the number of such cases at one time in the house, has rarely amounted to eight; and it is right to state, that they are treated in every respect like the general class of patients.

Cases of a manifestly incurable character are not admissible, but this rule can never be strictly enforced. Persons subject to epilepsy are excluded by the same regulation, but they sometimes get admission from the existence of the disease being concealed.

The plan of the building admits the formation of five classes for each sex, and a separate airing ground is appropriated to each division. An important addition has lately been made of a large piece of ground, and including the gardens originally attached, there are now nearly twenty English acres belonging to the asylum.

The house was planned originally for the reception of 236 patients, but by alterations subsequently made, 288 can now be admitted; and yet, owing to the accumulation of incurable cases during a series of years, the accommodation is found to be inadequate to meet the wants of the district, and the asylum labours under the disadvantage of not being at all times able to receive patients immediately on their being attacked, a circumstance which has an important effect on the result of treatment. I am happy to add, however, that this defect is likely soon to be removed, as Government has sanctioned the addition to the building of accommodation for 100 patients.

My Report embraces a period of five years, commencing January, 1833, and ending December, 1837; and I purpose

confining my observations to the patients admitted during this time.

The population of the district, according to the census of 1831, amounted to 803,396, and each county being charged with the expense of the patients received from it, I am enabled to compare the numbers with the population of each division respectively.

Admitted from	Population.	Number of Patients.	
		Males.	Females.
City of Dublin	204,155	167	128
(Within the ancient boundaries.)			
County of Dublin	176,012	68	80
County Meath	176,826	40	25
County Wicklow	121,557	25	23
County Louth	107,481	27	19
Town of Drogheda	17,365	4	2
Total,		331	277

The large number of patients admitted from the city and county of Dublin, as compared with the other divisions, shews the much greater prevalence of the causes of insanity in cities and thickly populated neighbourhoods, than in the rural parts of the district. The town of Drogheda is rather an exception to this rule.

The medical certificate required with each patient contains a query as to the probable cause of the disease, where such could be assigned ; and from this source I have arranged the following table of causes :

	M.	F.		M.	F.
Intemperance and abuse of ardent spirits.	74	12	Grief from death of rela- tives	11	18
Pecuniary losses and re- verses of fortune	35	20	After fever	10	8
External injuries from falls, wounds, &c.	18	4	cholera	1	1
Destitution and want of employment	13	10	influenza	1	
Domestic disagreements and bad treatment by relatives	9	10	scarlatina		1
Love and disappointed af- fections	5	18	erysipelas		1
Jealousy	10	10	Apoplexy	2	
Seduction		1	Dread of cholera		1
Religion	10	18	Abuse of mercury	10	2
Pride	1	1	Fright	4	26
Excessive fondness for music	1		Hereditary predisposition	8	5
			Close confinement and study	7	
			Political excitement	1	
			Puerperal state		22
			Violation of the person		3
			Change of life consequent on marriage		1

This table shews a greater proportion of physical causes amongst men, and of moral causes amongst women, not exclusive of those peculiar to the latter sex. Besides the large number of cases attributed to intemperance, I have no doubt that the same cause operated remotely in many others, by leading to those circumstances from which the disease was supposed more immediately to proceed. Of the cases denominated religious, I believe but a very small number could fairly be attributed to religion as a cause; religious enthusiasm and religious despondency, though often mistaken for the causes, are much more frequently the consequences of insanity. The puerperal cases occurred either during pregnancy or very soon after delivery; in some few of them, other causes may have been combined with the puerperal state, such as fright and family disagreements. One woman who was attacked immediately after the birth of

her first child, had been insane before marriage. In addition to those cases stated as hereditary, wherein this was the only cause assigned, nineteen others were noted, one or other of whose parents had been insane ; and in forty-four more, insanity had shewn itself in collateral blood relations. Amongst the cases for which no satisfactory cause was assigned in their certificates were two young females, aged sixteen years, in whom the catamenia first appeared after being admitted into the asylum, and they speedily recovered on the establishment of this discharge. One was readmitted at the end of eight months, and the other after an interval of more than three years, both being affected with suspension of the catamenia. Suitable means being employed for the restoration of this evacuation, every symptom of insanity soon disappeared. In many other cases, irregularities of the menstrual function existed ; in some, perhaps, the consequences, in others, very probably, the cause of their insanity ; but in none was the connexion of the two affections so strikingly exhibited as in the two first mentioned.

Before quitting this part of my subject, I wish earnestly to invite the attention of gentlemen who may be called on to sign certificates of insanity, to the advantages of accuracy in filling up answers to the queries contained in them, as the medical officers of the asylum have frequently occasion to regret the want of correct information as to the history of cases.

The occupation or condition in life of each patient is shewn in the table following.

MALES.		FEMALES.	
Clergymen	3	Governesses	4
Apothecaries	2	Schoolmistresses	3
Attorneys	1	Shopkeepers and small	
Traders and shopkeepers .	23	traders	22
Schoolmasters	5	Dressmakers and other	
Mercantile and law clerks	27	female trades	45
Artisans and tradesmen .	132	Domestic servants	69

MALES.		FEMALES.	
Servants	32	Washerwomen	5
Farmers	11	Wives or daughters of	
Soldiers and pensioners .	9	tradesmen	53
Sailors	9	Do. of professional men	5
Police and revenue officers	6	Do. of farmers	11
Labourers	60	Do. of labourers	41
Mendicant	1	Do. of clerks or stewards	9
Unknown	10	Mendicant	1
		Midwife	1
		Prostitutes	3
		Unknown	5

The class of labourers, the lowest in the social scale, though numerically greater than all the others, does not furnish a fourth of these cases. As we ascend in society, mental cultivation is greater, more means of vicious indulgence exist, and greater liability to the vicissitudes of fortune ; and we also find a greater proportional number of cases of insanity ; not, indeed, in a regularly progressive ratio, but speaking generally, in the middle and upper classes of society, insanity is more common than in the lowest.

In my inquiries respecting the condition in life of the females, I was surprized to find so small a number of prostitutes, as the contrary might be expected, on considering the excesses to which they are addicted, and the moral and physical evils to which they are exposed. Only three have been admitted within the last five years ; and I learn that the average number in former periods was not greater. It is right to anticipate an objection that may be made, by stating, that women of this class, when proper subjects for the asylum, find as ready admission as any others. I should not have alluded to this circumstance, but that it is at variance with reports of lunatic asylums elsewhere, particularly in Paris. It has been stated by Esquirol, that at the Salpêtrière, the great hospital for insane females, one-twentieth of

the whole number had been prostitutes. And Parent Duchatelet, in his extraordinary work on prostitution in the city of Paris, states that 105 cases of insanity had occurred amongst prostitutes in the course of five years. Why women of this description in Dublin should be more exempt, I can assign no satisfactory reason. The peculiar manners and habits of the same class in the French capital, as described by Parent Duchatelet, will probably account, in some degree, for their greater liability.

Of the men, 126 were married, twelve were widowers, and 134 were unmarried. Ninety-eight of the females were married, twenty were widows, and 115 were unmarried. The state of the remainder was not ascertained, as the query on this subject was not contained in the certificates at the commencement of the period embraced in this Report. An account being kept of the religion of the patients; of the 608 under consideration, 146 were Protestants, and 462 Roman Catholics, being in the proportion of one of the former in every $4\frac{1}{6}$; which is greater than that of the respective numbers in the population at large, wherein, according to the census, Protestants constitute one in every $5\frac{1}{4}$. But as all classes are included in this calculation, a fairer comparison would be of the patients in the asylum with the admissions into a general hospital indiscriminately open to the lower classes. I have, accordingly, procured returns from two of the hospitals connected with the House of Industry, of the patients admitted in one year, and I find the Protestants to have been one in every seven. I am, therefore, warranted in saying, that relatively to the respective numbers in the population, there are more insane Protestants than Catholics. This difference I conceive to have very little connexion with religious belief, but is mainly, if not entirely attributable to the fact, that the great disparity in the numbers of Protestants and Catholics in this country exists in the lowest classes of society, and that in ascending the scale, the numbers approximate; and as it has already been stated, insanity is least prevalent in the lowest

classes. It is not meant by these observations altogether to exclude religion as a cause of insanity; and perhaps cases of a purely religious origin may be more common amongst Protestants, than Catholics; they are, however, only of rare occurrence.

TABLE OF AGES.

	Under 20 years.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	Above 70.
Males . .	17	85	106	75	29	16	3
Females .	20	85	86	42	28	10	6
Total . .	37	170	192	117	57	26	9

487 were admitted in the first attack, seventy-eight in the second, twenty-four in the third, fourteen in the fourth, four in the fifth, and one in the sixth attack of insanity.

The duration of the disease before admission is shewn in the next table.

ADMITTED IN THE MONTH.											IN THE YEAR.				
	1st.	2nd.	3rd.	4th.	5th.	6th.	7th.	8th.	9th.	10th to 12th.	2nd.	3rd.	4th.	5th.	6th.
Males.	78	69	33	30	26	10	17	5	7	9	17	15	12	3	6
Fem.	50	47	32	28	12	7	13	5	4	12	24	17	9	6	11
Total.	128	116	65	58	32	17	30	10	11	21	41	32	21	9	17

It has sometimes occurred that a delay in the admission arose from the want of sufficient accommodation in the asylum; much more frequently, however, from neglect of timely application. The friends of patients often dislike their being placed in an asylum so long as they are manageable at home, and in consequence, that period of the disease is too frequently allowed to pass over when proper medical treatment is most likely to be efficacious. The long continuance of the disease in a great many cases precluded the hope of much benefit being derived from a residence in the asylum beyond that of safe-keeping. This circumstance, as well as the advanced age of many patients

should be taken into account in estimating the results of treatment as shewn underneath.

DISMISSED.					
	Recovered.	Relieved.	Unrelieved.	Died.	Remaining.
Males . .	150	29	10	57	85
Females .	116	44	3	31	83
Total . .	266	73	13	88	168

From the recoveries, forty-two must be deducted on account of re-admissions in consequence of relapses ; thus reducing the number of permanent recoveries to 224, so far as the event is known. But on the other hand, credit is to be taken for at least one-half of those discharged as relieved, who are known ultimately to have recovered ; the removal of a patient from the asylum who is tranquil and easily managed, having often a salutary effect. Again, of those remaining, a very considerable number are likely to recover, which circumstance must be considered in forming any calculation of the curability of the disease.

The length of residence in the asylum of the cases of recovery was as follows :

MONTHS.																							
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	22	26	28	34	41
53	51	30	22	19	13	16	19	7	7	4	4	2	4	2	4	1	1	2	1	1	1	1	1

enter upon the pathology of insanity, the following table is merely intended to shew the probable causes of death, not to exhibit a view of all the morbid changes in the different organs. Post mortem examinations were made in a great number of cases, but we had sometimes to regret that the interference of friends prevented these researches. In such cases of paralysis as were examined, chronic arachnitis was invariably found, with more or less serous effusion, and hyperemia of the substance of the brain. The large proportion of deaths from head affections in males, is confirmatory of what has already been stated of the greater prevalence of physical causes of insanity in them than in females, and also explains the greater mortality. All the other diseases are such as might be met with amongst the same number of individuals under different circumstances.

Died in consequence of

	M.	F.		M.	F.
Inflammation of brain or			Chronic enteritis	3	3
its membranes	11	1	Chronic peritonitis	1	1
Ramollissement of brain .	1		Mesenteric disease		1
Chronic hydrocephalus . .	1		Diarrhoea	2	2
Paralysis	9	1	Fever	4	2
Apoplexy	3		Cholera	1	
Epilepsy	3	2	Erysipelas	1	
Phthisis	7	7	Psoas abscess		1
Bronchitis		2	Gangrenous ulcers	2	1
Pneumonia	1	1	Abscess of kidney	1	
Organic disease of heart .	1		Old age and general de-		
			bility	5	6

I subjoin the ages at which death occurred in the fatal cases, as bearing upon the question of the tendency of insanity to shorten the duration of life, which is generally considered in effecting life insurances.

Died

Between 20 and 25 years.	25 and 30	30 and 35	35 and 40	40 and 45	45 and 50	50 and 55	55 and 60	60 and 65	65 and 70	70 and 75	85
3	9	11	14	10	9	7	7	9	3	5	1

The important subject of the medical treatment of insanity does not come within the scope of this paper, but as connected with statistics, and being at the same time a very material part of the treatment, I have yet a few observations to make on the occupations provided for the patients.

The advantages of laborious employment in the treatment of insanity have long been known, but its systematic adoption in public asylums is of recent date. I have already mentioned the extent of ground attached to the asylum; the part of this lately acquired, is now being converted into a market garden for the growth of vegetables for sale. An average number of sixty men are constantly employed in the cultivation of the grounds; and although entrusted with the use of spades, shovels, and other implements, no serious accidents have ever occurred. They are of course at all times under proper superintendence. About fifteen men find employment in various trades, as weavers, tailors, shoemakers, and carpenters. A few are occasionally engaged in breaking stones, and making mats, others in the performance of various domestic offices in the house. The female occupations consist of spinning, knitting, and the various branches of needle-work, and they assist in washing, and in all the offices of house-maids. All the clothing for both males and females is made up by the patients, with the exception of hats and shoes. During the last year, 2088 yards of linen and calico were woven in the establishment, the yarn for the linen having been spun by the females, and 524 pairs of stockings were made by them. Small rewards are generally given for the articles manufactured, which here as elsewhere operate powerfully as a stimulus to exertion. Of the advantages of this

system of employment, I can speak in very decided terms; persons who, when unemployed, are noisy, violent in their demeanor, quarrelsome, and discontented, very generally become, under the influence of suitable occupation, tranquil, orderly, and easily managed; and to its salutary effects I mainly attribute the recovery of numerous cases, some of which were at first most unpromising. Difficulties will no doubt occur in its individual application, requiring perseverance and address, and of course, it is not suitable to every case, nor to every stage of the disease; but I can confidently say, that at least eight out of every ten lunatics will be found in a fit state for some useful pursuit; and it is often a subject of regret that our means of employment are inadequate. The advantages, in an economical point of view, of employing the patients in a pauper establishment, are so obvious as to require no comment.

I shall give a brief sketch of a few cases in illustration of these observations. A farmer about twenty-seven years of age was admitted into the asylum, after having been nearly four years in a state of dementia, in consequence, as was reported, of the improper use of mercury. He was listless and taciturn, and would only reply to questions about himself, by telling his name and place of abode; his general health was unimpaired, and his appetite was good, but before commencing his meals he always required the assurance of some of the attendants that the food was good, and proper for him to eat. At first, he refused to engage in any occupation, but being otherwise amenable, and shewing no vicious propensity, a wheel-barrow was placed before him, and a man at either side took him each by a hand, and fixing them on the handles of the barrow, he was gently urged to the use of it; finding this plan persevered in, he soon consented to work by himself; and afterwards taking to the use of the spade he proved himself an excellent workman. In a short time an improvement was apparent in his mental state, he became gradually more intelligent and communicative; his recovery proceeded without interruption, and

he was discharged quite well after a residence of eleven months in the asylum.—A tailor, aged thirty-six years, was admitted in a state of melancholy of six months' duration; he was remarkably timid and reserved, and when questioned about himself he seemed disposed to give the required information, but his sentences were always unfinished from failure of memory, and want of words to express himself; his general health was impaired, and his appetite poor. After having been for some time under medical treatment, it was determined to try if he could be induced to work at his business, another tailor being constantly employed in the same division with him. Some difficulty occurred in persuading him to make the effort; but by encouraging language, and holding out the hope to him that he should be enlarged as soon as he shewed that he was able to resume his trade, he was led to begin. At first his progress was slow, and frequently interrupted by fits of abstraction; but he gradually became more collected, and capable of longer continued application, and by steady perseverance in this course he recovered perfectly, and was discharged nine months after his admission.—A man, aged twenty-eight, who had served for some years in the East India Company's Artillery, was invalided in the island of St. Helena, and discharged as unfit for service in consequence of insanity, which had commenced with delirium tremens. He was admitted into the asylum about twelve months after being first attacked, when he was incapable of giving any correct account of himself; he was melancholic, and in general silent; when he did speak, his conversation was an incoherent jumble of the recollection of past events; his general health was a good deal impaired. For nearly three months after his admission there was very little change in his situation, he then began to shew more intelligence, and to take more notice of what was passing around him. It was discovered that he had been accustomed to the care of horses, and was fond of them; advantage was taken of this circumstance, and he was employed about the manager's horse, which in a short time was

entrusted entirely to his care. From this period he steadily improved, and his recovery was complete in twelve months from the time of his admission. After being discharged as a patient, this man was employed as a keeper in the asylum; the duties of which situation he has now performed in a very satisfactory manner for the last two years. I may here mention, that one of the best nurses at present in the establishment is a woman who first entered it as a patient many years ago, and was then in a state of violent excitement.

The example of others employed around them greatly assists in overcoming the disinclination to every kind of exertion which many patients evince; and although the results of employment will not be always as satisfactory as in the examples I have adduced, still in every case more or less advantage will be derived from it. The general health is promoted, discipline is maintained, and active exertion in the open air is found more advantageous in producing quiet sleep than opiates. Many of our most industrious patients are persons whose cases are likely to prove incurable, but who are made as happy as their several states admit of; some, by their labours, are fully requiting, others materially lessening the expense of their maintenance to the public.

A Protestant and a Roman Catholic chaplain are attached to the institution, and thus, every inmate capable of benefiting by religious consolation has the means provided of doing so. No unnecessary coercion is ever employed, and every patient is allowed as much liberty as is compatible with his own safety and that of others, and it rarely happens that the number of those under personal restraint amounts to two in 100. Distinguished physicians from various quarters, who have visited the asylum, have expressed unqualified approbation of the extreme degree of cleanliness and order apparent in it, which are indeed such as reflect the greatest credit on Mr. and Mrs. Wrigley, the manager and matron; and I have great pleasure in bearing my testimony to their unceasing exertions in promoting the comfort and well-being of the patients in every particular.

I cannot conclude this Report without congratulating my friend Doctor Jackson on the present state of the institution; having suggested its origin, and assisted in planning it, and having been the chief medical attendant since its opening, he has now the pleasure of seeing it attain such a degree of perfection as admits of a comparison with any similar establishment to be found elsewhere.

To the Board of Governors it must be a subject of sincere gratification to see their kind intentions fully carried into effect in the treatment of a class of individuals deserving of sympathy above all others who are the objects of public charities. The consciousness of well directed exertions in the cause of benevolence, and the gratitude and esteem of their fellow-citizens, are their sole reward.

APPENDIX.

In the following tables, a general view is given of the total number of lunatics confined in district asylums, and also in the various other places for their reception, public and private. I have taken the numbers chiefly from the annual Report made to the House of Commons, by the Inspectors General of Prisons and Lunatic Asylums, the latest published being up to January 1st, 1837.

DISTRICT ASYLUMS.

Site.	Extent of District.	Population of District.	No. in each.
Armagh	Armagh. Cavan. Monaghan.	793,336	115
Limerick	Limerick. Clare. Kerry.	836,803	290

Site.	Extent of District.	Population of District.	No. in each.
Belfast	Down.	677,627	167
Londonderry	Antrim.		
	Londonderry.	815,629	180
	Donegal.		
	Tyrone.		
Maryborough	Queen's Co.	539,506	131
	King's Co.		
	Westmeath.		
	Longford.		
Carlow	Carlow.	566,811	129
	Kilkenny.		
	Wexford.		
	Kildare.		
Ballinasloe	Galway.	1,343,914	145
	Mayo.		
	Leitrim.		
	Roscommon.		
	Sligo.		
Clonmel	Tipperary.	402,563	83
Waterford	Waterford,	177,054	86
	City and Co.		
Dublin	Dublin, City		
	and County.	803,396	284
	Meath.		
	Louth.		
	Wicklow.		

Of the patients in the District Asylums there are

Idiotic,	136
Epileptic,	68
Reputed curable cases,	525
Incurable,	881
Total,	1610

In consequence of the crowded state of many of the asylums, there are at all times outstanding applications for the admission of patients, the number of which may be estimated at about 120.

Number of Lunatics confined, but not in District Asylums.

Site.	Denomination.	No. in each.
Dublin	House of Industry.	474
	Swift's Hospital.	148
Cork	House of Industry.	368
Clare	Infirmary.	7
Lifford	Local Asylum.	14
Kilkenny	County Asylum.	30
Limerick	House of Industry.	65
Londonderry	Infirmary.	10
Clonmel	House of Industry.	23
Waterford	House of Industry.	48
Wexford	House of Industry.	30
Various counties	Gaols.	79
	Private Asylums.	223
	Total, . . .	1519

Of these cases there are

Idiots	392
Epileptics	211
Reputed curable	254
„ incurable	662

The lunatic department of the House of Industry, Dublin, is appropriated to the reception of incurable lunatics and idiots; into this as well as into Swift's Hospital, patients are admissible from all parts of Ireland. The Cork Asylum, though not classed with the district ones, having been established previous to them, is appropriated to the county and city of Cork, the population of which amounts to 810,732. The lunatics confined in gaols are either criminals preparatory to trial, or are so placed before removal to the district asylums. Their presence in the gaols is found to be attended with so much inconvenience, that a remedy is about to be applied by parliamentary enactment.

Of the patients in Swift's Hospital, ninety-five are of the

pauper class, and supported by the funds of the charity, the others are boarders, who pay for their maintenance. In all the other places of confinement, except the private asylums, the patients are supported at the public expense.

ART. XVI.—*Some Observations on the Diagnosis of Cynanche Trachealis and Laryngitis.* By TRAVERS R. BLACKLEY, A.B., M.R.I.A., Member of the Royal College of Surgeons in Ireland; and Medical Superintendant of the Hospital for Children, Portobello.

As the value of any particular form of treatment in disease depends mainly, if not entirely, on the accuracy of the diagnosis which we form, and as many diseases, essentially different in their nature, are represented for the most part by the same train of symptoms, we cannot be too careful in arriving at correct conclusions respecting them; but if this is sometimes found difficult in affections of parts even at a *distance* from each other, how much more complicated may the case become when the *same* part is affected by either of two different diseases, each attended by nearly the same symptoms, but requiring, it may be, a very opposite line of treatment. To this may be added another source of embarrassment, namely, when the *age* of the patient seems to predispose powerfully to the one form of disease, and to exempt it in all probability from the other. There are perhaps few cases in which these observations apply more forcibly than in the “laryngitis of children,” the symptoms of which disease, together with the age of the patient, combine in no slight degree to deceive even the most attentive practitioner, and to impress upon him the belief, that the affection is indisputably one of croup, or cynanche trachealis.

Laryngitis is of three kinds, and although in their symptoms there exists a striking similarity, yet in their causes and pathology we shall remark the most essential differences. 1st. Laryngitis

caused by effusion under the mucous membrane, as described by Dr. Cheyne, the symptoms of which he describes as follows:—"Change in the sound of the voice; difficult and even crowing inspiration; an altered, sometimes stridulous voice; fits of suffocative coughing; and all those symptoms which arise from obstructed circulation in the lungs." As to the pathology of the disease: "the following appearances," he says, "are always visible. The epiglottis thickened and erect, by which it ceases to protect the aperture of the wind-pipe; the mucous membrane of the glottis and larynx, as well as the epiglottis, thickened and vascular; underneath the mucous membrane an infiltration of serum. This thickening of the mucous membrane, and distention of the sub-mucous tissue from inflammation and effusion, bring the sides of the *rimæ glottidis* nearly into contact, and thus at length almost obliterate the passage." This is the disease to which the famous General Washington is supposed to have fallen a victim, and is one essentially of adults; perhaps we may, with propriety, call it "the *submembranous* (or *sub muco-membranous*) *laryngitis*." 2nd. That condition of the larynx in which it assumes the diphtheritic character (generally secondarily) from the *pharynx*, as in diphtherite *gangrenosa*; or the *trachea*, as in croup. This differs from the former species in having its seat on the free surface of the mucous membrane. 3rd. Acute inflammatory laryngitis, terminating in ulceration; this disease may appear as a secondary affection, as after measles, syphilis, small-pox, &c., or it may arise independent of any exciting cause with which we may be acquainted; most generally perhaps we shall find it to belong to the former class; of the latter, however, examples are not wanting, but the cases to which I desire to draw particular attention are those occurring in children, which, from the very considerable resemblance they bear in their symptoms to *croup*, may very naturally be referred to that disease. That this mistake has frequently been made I entertain little doubt, not only in this country, but in France, as I shall subsequently endeavour

to prove ; and it may serve to explain in a more satisfactory manner than has hitherto been done, why the operation of tracheotomy has been had recourse to with success in diseases *believed to be true croup*, (but which in reality were laryngitis,) contrary to the best received opinions of the pathology of that disease.

The importance of this subject has been strongly impressed upon my mind by a case which recently occurred to me and my friend, Dr. H. Montgomery of Belfast, and in which, I confess, I made the very mistake to which I have alluded.

John Cavanagh, aged four years, was admitted into the Hospital for Children, Portobello, on the 6th of November, having laboured under feverish symptoms for five or six days previously, the last two or three of which his breathing had become much affected ; his condition was as follows : his cough exhibited the usual croupy crow, when he spoke it was below his breath ; his respiration was rapid, irregular, and *stridulous*, and now and then interrupted apparently by collections of mucus in the throat, which he was unable to expectorate ; lips dirty, and tongue loaded with white sordes ; the amygdalæ covered with slimy mucus, and swollen, but not sufficiently to cause the dyspnoea ; the pulse remarkably quick, and of tolerable strength.

The case was looked upon as croup, and six ounces of blood were instantly taken from the arm, the child admitted into the hospital and put to bed ; immediately the breathing became fuller and more regular, and the countenance less expressive of anxiety and distress ; skin warm and moist ; pulse 148 ; respiration 33.

The bleeding was followed up by the tartar emetic mixture, which instead of producing vomiting, seemed to be determined rather to the bowels, causing tympanitis, and tenesmus, &c. To subdue the tympanitis an appropriate enema was ordered, while the emetic mixture was rendered more active by the addition of some hippo wine, which had the desired effect of producing some vomiting. On the following morning (the 7th)

his symptoms were aggravated, and he appeared much worn down; it was judged that the false membrane was formed, that the case was hopeless, and that palliatives alone were indicated: with this view, and to meet the exhaustion, an ethereal mixture was prescribed.

On the following day (the 8th) he seemed to make a rally, and in hope of assisting the separation of the false membrane, it was judged advisable to put him on calomel; on the evening his breathing was so difficult, that in the expectation of affording relief, an emetic was given, but without any good result.

On the 9th a forcible examination of the throat being made, matter was observed to fill the back of the pharynx, and as he appeared unable to expectorate it, it was removed mechanically, in the hope of giving him relief. An astringent gargle was used to facilitate, if possible, the separation of the false membrane, a portion of which was supposed to be detected in the sputa, and the existence of which not only confirmed the belief that the disease was croup, but also served as a sufficient reason for abstaining from the operation of tracheotomy, had a contrary opinion been entertained.

The child died, and had we been precluded from making a *post mortem* examination, we should have remained convinced that the case had been one of croup in an aggravated form, and that the morbid appearances to be observed would have been simply those of that disease, namely, false membrane, with traces of active inflammation, as described in some of the severest cases of croup. An examination, however, was procured, and the result proved how defective would have been such a conclusion.

Examination sixteen Hours after Death.—There was no tumefaction externally; the thyroid veins were turgid, but not enlarged. On cutting out the upper part of the trachea and the larynx, about half an ounce of pure pus was effused in the proximity of the latter. Looking through the tube from be-

low upwards, the larynx was observed to be impervious, owing to a collection of purulent matter; slitting open the trachea posteriorly, and dividing the cricoid cartilage, the epiglottis, on its apex and inferior surface, the aryteno-epiglottidean folds, and the mucous membrane covering the arytenoid cartilages, were found extensively ulcerated; the membrane of the sacculus laryngis was completely disorganized; the ulcerated rima glottidis presented a ragged and unequal edge; and the mucous membrane, in connexion with the ulceration, was highly inflamed, and small ulcerated lesions were dotted over it; some of the mucous glands also, on pressure, gave out pure pus. The ulceration terminated inferiorly in two semilunar margins, corresponding to the inferior edge of the thyroid cartilage, below which the trachea was but moderately inflamed. The *nature* of the disease therefore was mis-apprehended, and what was really a case of pure *laryngitis*, terminating in ulceration, was supposed to be croup. Taking the whole history of the case, and the symptoms which presented themselves, into consideration, the mistake was a natural one, but when to these we add the *age* of the patient, which greatly predisposed to the latter, and rendered most improbable the occurrence of the former disease; few practitioners would have formed any other diagnosis than that at which we arrived. Though the result (even if the exact nature of the disease had been ascertained during life) would have been problematical, I yet feel that the operation of tracheotomy should have been performed in this case; it would have prolonged life, to some extent at least; and if nature gave her assistance in speedily healing the ulcerated parts, would have preserved it; but as this is an operation exploded in true croup, though perhaps the best practice that can be pursued in cases of laryngitis, and as the symptoms of the two diseases are in a great measure similar, and many of them *identical*, it becomes a question of the greatest importance, "*what are the true diagnostic signs by which we shall be able to distinguish the one affection from the other?*"

The first to which I shall direct attention is, the existence of fever for a few days before the more alarming symptoms are developed. 2nd. The fever is *continued*, and becomes gradually aggravated. 3rd. Pain in deglutition. These many be considered as the most positive marks by which laryngitis is distinguished from croup; to these we may add hoarseness, or total loss of voice, pain on pressure externally, and constricted feeling in the throat; inflammation at the back of the pharynx, the hawking up of thick tenacious mucus, as mentioned by Dr. Good, a symptom by-the-by, much more apparent in the adult than child, and anxiety for open air, and change of situation.

Dr. Copland, in his Dictionary of Practical Medicine, writes as follows:—

“Laryngitis is with greater difficulty distinguished from croup, and in many respects there is little or no difference. The practical importance of the diagnosis may not appear great, but it is sufficiently so to warrant an accurate distinction. 1st. True laryngitis occurs in adults, seldom in children in any other form than associated with either the simple or complicated states of croup. 2nd. It is a purely inflammatory disease, attended by a fixed burning pain in the larynx, increased on pressure and examination, and when attacking adults never gives rise to a false membrane, unless it be superinduced in the specific and epidemic forms of cynanche, and then it assumes modified characters. 3rd. It more frequently terminates in the manner characterizing acute inflammation, viz.: ulceration and suppuration, than when the larynx is affected in croup. 4th. It is more acutely and constantly inflammatory, the symptoms are more continued, and it is more benefited by a purely antiphlogistic treatment than croup. 5th. It much oftener passes into the chronic form than the latter disease.”

Our French brethren deserve the credit (if any) of reviving, in these days, the operation of tracheotomy in cases of croup, not only with the view of facilitating respiration, but of removing the false membrane, and not only this, but also of applying such

remedies *topically* as circumstances may require. To M. Bretonneau is due the praise of this bold treatment, which it appears has been frequently successful in his hands; Velpeau in his "*Medicine Operatoire*," commends him highly; and perhaps I cannot do better than translate a few passages from the latter work in order to explain the motives and manner of such practice.

"Persons in croup, on account of their inability to breathe freely, die in a state of asphyxia. This asphyxia, frequently caused by the presence of a false membrane, or the puffing of the laryngeal membrane, never depends on spasmodic lesion, which the cartilaginous texture of the bronchi, the trachea, and the larynx renders impossible, or at least insignificant. It is not so much to extract the membranous concretions, that we should have recourse to bronchotomy, as to gain time, and put the patient in a condition to respire, while we decide on the means of curing him. Monsieur Bretonneau has proved, moreover, that once the trachea is opened, there may be driven into it a solution of calomel with advantage, or even a solution of nitrate of silver may be introduced in a little sponge fixed to the end of a slip of whalebone, and thus follow the false membrane as far as the bronchi; in a word, treating the diphtherite of the *trachea*, as he has, with so much success treated that of the pharynx, (*l'arriere gorge*.) In this respect, bronchotomy is a valuable resource, which ought to be employed wherever the disease extending into the larynx or below it, (but not having yet reached the bronchial division,) topical applications can no longer be made by the mouth. Four unhoped-for cases of success support this practice. In the month of July, 1825, M. Bretonneau called on to attend a Miss De Puysegur, aged four years, whose three brothers had already died of croup, and who was herself affected by it in the last degree, made a large opening into the trachea; placed a canula in the wound; observed false membrane to escape from it in considerable quantities during many days; blew into it some calomel

in powder, which was badly borne, afterwards some of the same substance diluted in water, and thus succeeded in saving this unfortunate child.

“In the case of a boy, aged seven or eight years, whom I myself examined at Tours, in 1827, a month after his being cured, and who, in the last stage of the complaint, was about to be left as dead by his relations, M. Bretonneau cut open the trachea as in the former case; he observed him at the expiration of a few minutes to revive; extracted numerous membranous concretions; a little later conceived it necessary to convey through the canula, which he had left in the wound, a solution of caustic, which he did with the assistance of a whalebone rod, armed with a very small piece of sponge; and after sundry difficulties, overcome as soon as perceived, the child was completely cured.

“Recently in October, 1831, the same practitioner was not less fortunate in a third case; the child, aged eleven years, was looked on as dead; when M. Bretonneau was called to him, he instantly opened the trachea, and after sundry incidents encountered by the best directed care, this young person has been perfectly restored.”

From the above description, we may fairly conclude that the two first cases were those of genuine croup; the last is rather loosely worded to admit of our arriving at this conclusion; but to the *fourth and last case*, I would request particular attention, because it appears to me, 1st, that it was *not* a case of croup; and 2nd, that it *was* one of *ulcerated larynx*, similar to that which I have myself detailed.

“Like success has been met with at Paris by M. Trousseau. A young boy of six years and a half old, felt the 21st of November, 1831, a pain in the throat rather severe, accompanied by cough, hoarseness, and some fever. On the 23rd, at nine o'clock in the evening, three physicians met in consultation; all were of opinion that the child was attacked with croup, and that death would unquestionably take place before two

hours; M. Trousseau proposed tracheotomy, and instantly performed it. The trachea was opened by dividing the cricoid cartilage to the extent of seven lines. The venous hæmorrhage stopped almost immediately; a good deal of blood, however, fell into the bronchi, which the child immediately returned by the wound, with some 'debris' of false membrane; *on the instant, respiration became perfectly calm*; a metallic canula was then introduced, like that which has been described by M. Bretonneau, in his treatise on Diphtherite, then twenty drops of a solution of nitrate of silver (3i. to 3i.) were dropped into the bronchi; those drops every six or seven hours were repeated during three days and a half, every hour likewise twenty drops of warm marshmallow water were poured in. The child did not cease bringing up diphtherite concretions until the fourth day of the operation. The canula was withdrawn and cleaned three times a day, during the time it was applied; it was cleared several times every hour with a little mop of horse hair. The air began to pass freely through the larynx on the tenth day, and on the twenty-fifth the wound in the integuments was completely cicatrized; the child enjoys this day perfect health."

With the greatest respect for the talents and observations of MM. Velpeau and Trousseau, I think it may be fairly questioned, that this was a case of genuine croup. The symptoms were those of laryngitis; namely, an acute pain in the throat, hoarseness, and *fever*, with some cough; (it is to be regretted that the account is not more explicit; thus we are not informed whether there were attacks of suffocative dyspnœa, which would have served to distinguish croup in some degree;) on the night of the second day after this, when it was not supposed the child could live two hours, tracheotomy was performed, and *instantly the respiration became perfectly calm*. Perhaps there could not be adduced a stronger proof than this, that the disease was situated in the larynx, for had false membrane been effused throughout the trachea, relief must have been but partial, not

complete. As to “*les débris*” of false membrane, I know from experience how a person may believe that he detects false membrane, where he *expects to find it*, particularly in the circumstances of performing the operation, where some confusion naturally ensues, and where the sputa are a mixture of blood, pus, and tenacious mucus; as to the diphtherite concretions which were brought up until the fourth day of the operation, I confess I cannot but regard them as sloughs arising from the application of so powerful a solution of nitrate of silver, which was no sooner laid aside, than the concretions disappeared likewise. Again, “the air began to pass pretty freely through the larynx on the tenth day.” Why not sooner? if the false membrane was entirely removed from the trachea on the *fourth* day, on what principle can we reconcile the disease continuing in the larynx *six* days longer? and finally, the cure to be effected on the twenty-fifth day. Every thing relating to this case serves to confirm me in the belief, that it was one of laryngitis. The previous continued fever, pain in the throat, hoarseness, and cough; the immediate and complete relief on the trachea being opened; the time which elapsed before the air began to pass freely through the larynx, (ten days;) and finally, the period required for complete restoration to health, are all circumstances which convince me that the mischief lay in the larynx, and in it alone. If this opinion be correct, it may serve to prove, 1st, that a case of laryngitis was mistaken for croup; and 2ndly, what is of infinitely greater consequence in a practical point of view, that in such cases, *tracheotomy* is powerfully indicated as the only possible remedy to which we can have recourse, and happily one which holds out every reasonable prospect of success; and this circumstance must impress us most strongly with the necessity of an accurate diagnosis of the two diseases, as however useful tracheotomy may be, and no doubt is, in cases of laryngitis, its result in croup I must consider as at least problematical, or justifiable only as a forlorn hope in a desperate

disease, which in its very nature may almost be considered as fatal.

In performing tracheotomy, the first and chief indication must of course be the averting of suffocation ; but a second and not less powerful one, as far as the ultimate success of the operation is concerned, and one which may have escaped the observation of many, is, that the larynx is thus left *in a state of rest*, and the curative process allowed to take place, a measure impossible to effect, so long as the part is dragged up and down by the action of the muscles, as is the case in obstructed respiration, and so subjected to continued displacement.

I believe Mr. Carmichael was the first practitioner to point out this effect, as one of the indications for performing tracheotomy in cases of venereal ulceration of the larynx ; he says, “ The presumed *modus operandi* of an opening in the trachea, as a remedy, is to allow the patient to breathe through the artificial opening, and permit the larynx to remain undisturbed by the presence of a constant current of air, and thus induce that favourable state of quiescence which is necessary to the healing of an ulcer in any situation.”*

Velpeau, speaking of the same operation, remarks : “ *L'air trouvant, au-dessous du mal, une libre issue, laisserait le larynx en repos, ne generait plus les efforts medicateurs de l'organisme.*” In performing tracheotomy therefore we must bear those indications in mind, and for the purpose of effecting them, make a free opening into the trachea, as otherwise a certain quantity of air passing by the larynx, the necessary state of quiescence will not be procured, and thus the desired object be but partially obtained.

* Work on Venereal Diseases, 2nd Edit. p. 213.

ART. XVII.—*Remarks on the Identity of Structure between the Kidneys and Epidermoid Glands.** BY ROBERT ELLIOTT LINDSAY.

THE subject of the following remarks is the strong connexion, or rather identity existing between the nature of two important classes of glands and their functions in the human body, namely, the kidneys and skin; or to speak more plainly, that the secretion of urine is analogous, not merely in a physiological, but also in an anatomical view, to the means pursued in the secretion of other fluids in different parts of the body, as for instance, the perspiration, or secretion from mucous membranes, &c. However, the accompanying remarks will be directed solely to the analogy between the kidneys and the secreting power of the skin; which problem, if satisfactorily solved, must necessarily embrace within its range, the nature of other secretions, whether proceeding from glands or extended surfaces. Although organs of secretion have not hitherto been demonstrated in some of the latter, being necessarily so minute, in consequence of their extension, as to elude the observation of man; yet, by every rule of inferential reasoning, we are warranted in concluding the perfect existence of such glands. For if we merely adopt as truths in science, what are only cognizable to the senses or axiomatic to the understanding, without suffering our minds to be guided by the powerful aid of inductive reasoning, an end must be speedily put to every advancement in knowledge, whether appertaining to the complicated organiza-

* It may be necessary here to mention, that the celebrated German physiologist, Müller, has treated on the identity in structure of all descriptions of glands. However, it is to be presumed that the following observations are not rendered thereby superfluous or misplaced, as they tend to bring *more particularly* before the observation, the connexion existing between two important divisions of secreting organs, both in a functional and anatomical view; which, by the numerous and almost perplexing varieties of disease they are subject to, render them appropriate and interesting subjects of discussion.

tion of man, or the more abstruse and loftier paths of general science. The importance of such a theory is evident, as it presents to our study not only the anatomical composition of such minute parts, but also the explanation of the numerous phases, which disease presents in those membranes. It is then much to be wondered at, if any mind could be found so methodical and plodding in its researches, which acquiesces in the received doctrine that both the glands of the skin and the kidneys, serve for the purposes of expelling from the frame, substances known to be pernicious in their effects, and that the retention of such secretions produces results similar in their consequences, without at the same time acknowledging the identity in structure of both sets of glands. But if it be here shown that not only a great, but nearly a coinciding similarity of composition exists between such glands, so far as can be at present known, together with the fact of the corresponding nature of both secretions; the same theory must necessarily be extended to secretions from serous membranes, &c., where such glands have not hitherto been detected; and in every case where the presence of fluids is accounted for, by the generally accepted doctrine of *arterial transudation*, that the theory of the presence of glands for their formation should be adopted.

Let us then in the first place direct our attention, as a preliminary step, to the nature of the secretions proceeding from the kidneys, and those glands in the skin alluded to by Breschet, and called epidermoid glands; and see how far they assimilate in that respect. The kidneys, as remarked by Dr. Elliotson, are the great outlets for *azote*, while the lungs and liver are for *carbon*, and in the composition of urine we perceive one essential substance called by M. Fourcroy *urea*; in this *azote* prevails to a great extent. Besides this there are other substances entering into the composition of urine; several of the phosphates, uric and benzoic acids, but to *urea* its characteristic properties are owing. This composition is often varied and changed by the peculiarities of climate or disease, which

here it is unnecessary to direct attention to, as the numerous diseases, and their causes, are sufficiently known, without demanding more than a mere allusion to them at present.

The secretion from the epidermoid glands, next demands our attention. An abundant vapour is constantly exhaling from the body on its entire surface, which is known by the name of the *insensible perspiration* ; this is called sweat when in greater quantity, and in a liquid form. The dependence between the secretion of urine and the perspiration is remarkable, as it has always been observed, that when the former fluid is scanty, there is a more profuse cutaneous perspiration, and *vice versâ*. The perspiration is in a great measure aqueous, but it, like the urine, holds in solution several salts, and it contains a portion of *carbonic* and *lactic acids*, and sometimes minute portions of *urea* and *uric acid*. It is evident then, from this cursory view, that the skin is a respiratory organ, without referring to some of the lower orders of animals, on the surface of whose bodies this function is performed. It is also found that the secreting power of the skin takes upon itself the part of an auxiliary to the lungs, as in hot countries, the quantity of carbonic acid gas formed in the lungs in a given time, was about one-third less than in a cold climate, while at the same time the respiratory function of the skin, both as to the quantity of the insensible transpiration, and the formation of carbonic acid gas, was very remarkably increased. Thus without advancing further, *the object* of the epidermoid glands and that of the kidneys is at once proved to be similar, namely, the separation of noxious ingredients from the blood, while they differ as to the nature of the substances they secrete, carbonic acid gas being for the most part expelled by the former, and azote principally by the latter. The similarity between the morbid affections of the frame produced by disarrangements in either system of glands, goes far to prove, that occasionally the substances eliminated by one, are, in consequence of diseased action separated by the other. Diabetes is of frequent occurrence only in cold and

damp climates, while it is a complete stranger to warm countries: and to counterbalance this we observe, that cutaneous affections seem to be indigenous to southern climes. It will be sufficient to allude to some of those diseases belonging exclusively to the skin, or sometimes, more accurately speaking, to the epidermoid glands. Leprosy, of which so frequent mention is made in the Sacred Writings, originated in Judea; Framboesia or Yaws, a species of excrescence like mulberries appearing in various parts of the body is endemial to the Antilla Islands; also the Elephantiasis rubra of Cayenne in addition to herpetic and psoric eruptions, which are both frequent and common among the inhabitants of southern latitudes. We find that the epidermoid glands are not exempt from some of those cutaneous affections, but as was observed by the late Dr. Wallace, some eruptions, such as lichen and strophulus, depend on an enlarged, inflamed and injected state of these bodies. Turning our attention from those local affections of the skin, let us inquire are there any constitutional symptoms important enough to demand attention. A parallel may be drawn between the fever and disarrangement of the frame resulting from protracted retention of urine within the bladder, and that of so frequent occurrence consequent upon obstructed perspiration. This is important, more especially when taken into consideration with the foregoing remarks on the secretions of both sets of glands. In urinary fever, there is a remarkable connexion between the functions of the kidneys and skin; the urinous and ammoniacal smell exhaled from the body, the yellowish and oily moisture from the skin, and other distinguishing marks, serve as powerful proofs of such a similarity, by one set of glands assuming, as it were, the duties of the other. In the course of those remarks to prove the identity existing between the anatomical structure of the epidermoid glands and the kidneys, the nature of the two secretions have been spoken of, as to their properties, chemical and otherwise, and in what manner they are connected; as also the importance and

necessity of those secretions to the frame, together with the effects produced either by their modifications or cessation, and the similarity between their morbid effects ; these have been lightly alluded to, as a prelude to the more interesting subject of their identity of structure. The anatomical similarity they bear to each other is necessarily not so clear and defined as could be wished, owing to our present limited knowledge of those parts, constituting the glandular structure of the skin ; however, the leading and principal points of composition can be shown, which taken into consideration with the collateral arguments advanced before, must irresistibly lead us to the conclusion, that the kidneys are, on a magnified view, of the same nature and structure, anatomically speaking, as the epidermoid glands, which latter are at present the particular subject of comparison.

The form in which the kidneys exist in the foetus, is worthy of attentive observation ; we there find that the several glandular bodies of which the kidneys are composed, appear in a separated and divided state, thus as it were in part demonstrating the nature of their ultimate composition, and in this manner approaching the structure of the extended minor glands throughout the frame. Breschet has, in his work on the Structure of the Skin, alluded to these glands ; and Dr. Wallace, in Lectures published in the *Lancet*, distinctly treats on the anatomical structure of these bodies as seen in the Negro's skin. In proceeding to the examination of these glandular bodies, for the purpose of proving a similitude between their structure and that of the kidneys, we are in the first place enabled in ourselves to observe a fluid exuding from the surface over these glands. This, according to the last mentioned writer, implies the probable existence of an excretory duct, which duct can be demonstrated. Here are two points of similitude, the glandular construction of both, and the existence of an excretory duct in both, together with the passage of a fluid through such channels ; the similarity of such fluids as

to their composition - and peculiarities having been already shown.

The structure of the glands themselves next demands attention. We observe that with regard to the distribution of the nerves of sensation to those glands, a striking similitude is observable. The experiment on the skin of the negro, of penetrating the orifices of the excretory ducts with a fine needle, which can be easily performed when those bodies are enlarged, as on a blistered surface, plainly proves that their sensibility is not great, as such can be done *without exciting pain*; but that they, or the surface upon which they are placed, appear to be highly *vascular*, for they ooze blood, when punctured, more freely than the surface which surrounds them. From these facts it must necessarily be inferred, that they are more richly and numerously supplied with vessels than with nerves. A powerful proof of their similarity is here related—the absence of much sensibility, which the kidneys are remarkable for; while at the same time can there possibly exist a more satisfactory evidence of their identity, than the vascular nature of the epidermoid glands so explicitly demonstrated, while on the other hand, the corticle or investing tunic of the kidneys, we know is composed entirely and solely of a congeries of blood-vessels. Another convincing proof of the vascularity of the epidermoid glands, exists in the diseases to which they are liable; several cutaneous eruptions having their seat in those bodies alone, as has been before remarked; in which cases these glands for the most part are in an inflamed and injected state. In addition to these facts, transparent fibres have been detected with a lens, running into those bodies through the substance of the rete, which Dr. Wallace considered to be vessels; all of which complication of structure evidently assimilates to the anatomical plan pursued in the component parts of the kidneys,—first by having a *vascular envelope*, which can even be discovered with the naked eye, and next in possessing on a diminutive or decreased scale, all the other peculiarities of those larger organs,

which, if the expression be allowed, are the mould in which all the innumerable minute glands throughout the frame are cast. Nor is it to be conceived, why there should be any difference in the essential characteristics of glandular bodies, if, on account of an economical law of nature, a secretion should proceed from a gland of a condensed form, as the liver or kidneys, and a secretion not inferior in importance, as to its effects on the human frame, proceeding from glands contained in a widely extended surface, as a serous or mucous membrane, but bearing, in their anatomical relations, marks of identity with those other glands of a condensed and consequently enlarged appearance ; while it must appear evident, that the very fact of their extent and number is, of course, the necessary cause of their diminutive size. Their number proves also their importance, and their being so happily adapted by some provisional law of our nature to the functions assigned them, is the reason they present to our view a thickly scattered collection of secreting bodies, instead of a condensed and compact gland.

In our studies in Pathology, numerous appearances present themselves to our observation, to account for the presence of which would be difficult, or rather impossible, without at the same time acknowledging the existence of those hitherto undiscovered but necessary glands. To illustrate this, it is sufficient to allude to the presence of blood sometimes found in the ventricles of the brains of those who die of apoplexy ; a circumstance unaccounted for by eminent physiologists, as after the most minute examination, no orifice or rent could be discovered, however trifling, in the blood-vessels of the brain, to account for its appearance, even by the doctrine of *arterial transudation*. Here then we are compelled to adopt the doctrine of a generality of glands, but of course the nature of the action of those glands, is still wrapped in that same obscurity, which impedes our researches into the nature of the nervous system in general.

ART. XVIII.—*Observations on* PROFESSOR HAMILTON'S *Deviations from the ordinary Mode of stating Practical Results*. By ROBERT COLLINS, M. D., Late Master of the Dublin Lying-in Hospital.

IN the preceding number of this Journal, a letter, addressed to the Editor by Professor Hamilton of Edinburgh, is published, in the form of a reply to objections which I felt it my duty strenuously to urge (immediately after the appearance of the second volume of the Professor's Practical Observations) against the *hasty*, and if *generally* acted upon, mischievous measures urged by him, for the *artificial dilatation* of the mouth of the womb within twelve or fourteen hours, and the *actual delivery* of the patient within twenty-four hours from the commencement of labour.

My observations were published in this Journal in March 1837, and I would now earnestly request the impartial reader to peruse that article, and I am convinced he will agree with me, that, in all the lengthy letters of Doctor Hamilton, amounting to about 160 pages of the original work, there is not the shadow of fact or sound reasoning to induce me to refrain from stigmatizing doctrines fraught with so much danger.

It is with extreme regret I feel myself thus pressed by Doctor Hamilton to revert to the subject, as respect both for himself and his years would cause me to overlook much; consequently I had resolved to take no notice whatever of *three* letters containing 100 pages, published in the London Medical Gazette, although these letters were sent to me by Professor Hamilton in the form of a pamphlet, with a most *flourishing* and *equally modest* title page, headed "Refutation" of the Objections urged by Dr. Collins, &c. &c. I was satisfied that the plain facts I had stated, and the happy results I had demonstrated from our hospital practice, would have more weight with my professional brethren than the mere dogmatic *asser-*

tions, without a semblance of *proof*, emanating from Doctor Hamilton's fruitful imagination. That I was not wrong in my decision I have ample proof in a letter to me from Professor Hamilton, which accompanied that published in this Journal by him, and as it in the clearest and most striking manner exhibits his so called "refutation" to be a *signal* failure, and at the same time explains to the profession at large, the *real* cause of his *again* attempting what has already overwhelmed him, even in the sight of his own pupils, I cannot avoid giving the following extract from it :

"Having explained those parts of my doctrines, to which you and Doctor Murphy have objected, in my letters to the Editor of the London Medical Gazette, I certainly considered that I had done my duty to the profession, and that I should not have had occasion to revert to the subject. But to my *mortification* and surprise I learned a few weeks ago, that some very respectable Irish students were *seriously offended* at my observations on your animadversions.

"This information led me to offer to the Editor of the Dublin Journal of Medical Science, an article in reply to yours of March last, which I have now forwarded."

Such is Doctor Hamilton's own account of the impression made by his long epistles, and I am charitable enough to believe, that the mortification he states he experienced was the cause of his now occasionally using language which I shall not venture to designate, and which must vastly *lower* the Professor in the opinion of every well regulated mind, and make him appear still *less* amiable in the eyes of the students whose youthful and unbiassed hearts are imbued with more liberal and finer sentiments.

I appreciate fully the candour with which Dr. Hamilton states in his letter to me the necessity of his offering *another* article in reply, and nothing but a sense of duty could have forced me to reiterate my objections both to his doctrines and practice on the points noticed. I shall now endeavour to prove

to Doctor Hamilton, that "thinking is very far from knowing," and to use soft words and hard arguments, is much better than to be deceived by our own vain opinions, as self-conceit always makes opinion obstinate. The Professor's perseverance in his own opinion, in opposition to almost all others, reminds me of the anecdote of the juror who appeared in court (after having been locked up for many hours) to complain of the *obstinacy* of his brother jurors, and when asked by the judge how many had agreed upon the verdict, his reply was the other *eleven* !

I shall first endeavour to show my professional brethren, that the *very foundation* upon which Doctor Hamilton builds his "innovations" on the management of the first stage of labour is *absolutely* and *diametrically opposed* to recorded *facts* as well as to sound reasoning, and that notwithstanding such precepts have been long taught by him, they are *unsound*.

The following are Doctor Hamilton's own words, upon the validity of which the key-stone of his *arch* ideas rests, and which, although inculcated by him for a series of years to some thousand pupils, he complains the practitioners in London, Paris, and Dublin *reject*.

"MANAGEMENT OF THE FIRST STAGE OF LABOUR.

"One of the earliest innovations in the treatment of human parturition, which I found good reason to introduce, was the limiting the duration of the first stage of labour to twelve or fourteen hours, whenever the uterine contractions continue to be regular and progressive; and I have stated in my Practical Observations, that the following are the *necessary effects* of the *protraction* of that process *beyond* the *time specified*.

"*Firstly*. The powers of the uterus may, in the second stage, be inadequate to the expulsion of the infant, with safety to its life, or to the future health of the mother.

"*Secondly*. After the birth of the infant, the uterus may

contract irregularly, so as to occasion the retention of the placenta.

“*Thirdly.* After the expulsion of the placenta, the contraction of the uterus may be too feeble to prevent alarming hæmorrhage.

“*Lastly.* Supposing the patient to escape all those untoward circumstances, febrile or inflammatory affections of a most dangerous nature may ensue from the previous protraction of pain, and irregular distribution of the blood.”

Such are the doctrines inculcated by Professor Hamilton, upon the *soundness* of which his “innovations” alone rest, and upon which the utility of his measures is based.

That Doctor Hamilton’s opinions are contrary to fact, the results of the records and practice of every hospital with which I am acquainted fully testify.

I shall, however, chiefly state my proofs in support of the validity of what I assert in opposition to Doctor Hamilton, from the Practical Treatise published by me in 1835, in connexion with the Dublin Lying-in Hospital ; as it is now so *widely* circulated, reference can be easily made, besides it is from it he attempts groundlessly and erroneously to illustrate his own imaginary views.

Had Doctor Hamilton even hastily looked at the results of our hospital practice, he surely would not have ventured to hazard the *crude* and positive assertions contained in all his letters : but this, although put to him in sufficiently strong language in my Observations in this Journal, in March 1837, he in every single instance uncandidly withholds and refuses.

I shall now as briefly as possible *prove* to the satisfaction of every thinking individual, and that from the actual results of *sixteen thousand four hundred and fourteen deliveries*, that where the patient is properly treated during the progress of labour, the *mortality* from the effects of *protracted labour* is *strikingly small*.

Of the 16,414 women delivered, 164 died, or in the proportion of *one in one hundred*. FORTY-TWO only of the 164 who died, were more than TWENTY HOURS in labour. When we reflect, in connexion with this simple statement of *facts*, that only *forty-two* women died who were above *twenty hours* in labour, out of the vast number of 16,414; and at the same time look to the *cause* of the fatal result, (as given in my *previous* communication,) the truth of my assertion is, I think, *unquestionable*.

I have stated, that in the 16,414 delivered, the mortality was 164, or in the proportion of *one in 100*. I have *proved* elsewhere,* by an accurate table shewing the *cause* of death in each, that if we deduct the deaths from *puerperal fever*, which may be considered *accidental*, the proportion becomes greatly diminished, viz. to *one in 156*; and again, if we subtract those deaths from causes *not the results* of child-birth, (and which are marked in the table,) the mortality from effects arising in consequence of *parturition* is vastly reduced, viz. to *one in 244*.

No fewer than NINETY-SEVEN out of 164 deaths, arose from causes *not the results* of *child-birth*. Do not these calculations establish to actual demonstration, that the deaths in consequence of the labour being protracted or laborious are *trifling* indeed.

In *support* of this position, it is only required to examine the mortality in the hospital for the *four last years* of my residence after *puerperal fever* disappeared.† In this period, the very great number TEN THOUSAND SEVEN-HUNDRED AND EIGHTY-FIVE deliveries occurred; out of which *fifty-eight* died, which is nearly in the proportion of ONE in every ONE HUNDRED and EIGHTY-SIX.

I ask Dr. Hamilton does he *know*, or did he ever *hear*, of any even *distant* approach to this *favourable*, and I will add, happy result in 10,785 deliveries! The annals of medicine

* See this Journal, March, 1837.

† See p. 387 of Practical Treatise.

afford *none* ; and I fear not when we succeed in “ *opening the registry*” of the Edinburgh Hospital, the practice of the Dublin Lying-in Hospital will still retain its majority, and that our opposition to Dr. Hamilton shall be declared neither frivolous nor vexatious.

I would hope that Dr. Hamilton will at least do us the justice to acknowledge, what his own candour must dictate, with regard to the mortality in our hospital ; and that in this way I may have the benefit of his evidence in the *most satisfactory* manner against one and all of the doctrines he has advanced.*

When I again declare, that no artificial interference was attempted to effect the delivery, until the *safety* of the patient demanded such ; it must be evident to every impartial mind, that I am fully warranted from the results shewn, in stating my decided conviction of the *unsoundness* of any advice calculated to encourage rashness and *unnecessary intermeddling* on the part of the medical attendant.

Two “ untoward circumstances” are expressly set forth by Dr. Hamilton as the “ *necessary effects*” of the protraction of labour beyond the time specified by him ; upon which entirely rests the validity of his reasoning ; these constitute the *second* and *third* head of his doctrines, we shall use his own words.

“ *Secondly*. After the birth of the infant, the uterus may contract *irregularly*, so as to occasion retention of the placenta.

“ *Thirdly*. After the expulsion of the placenta, the contractions of the uterus may be too feeble to prevent *alarming hæmorrhage*.”

The *total* cases met with of retention of the placenta in consequence of *irregular* action, during my residence as Master in the hospital, were nineteen out of 16,414.

Of these nineteen, the *duration* of the labour is accurately

* I shall make some comparison with the Edinburgh, and similar institutions, so far as has been recorded, before I conclude.

recorded in seventeen ; *fourteen* of which were delivered within *twelve* hours ; *one* in fourteen ; *one* in twenty-four ; and *one* in sixty hours. Of the two cases not specified ; *one* had been in labour before admission, and was delivered immediately ; the other, the length of the labour was not noted.

The *total* cases of hæmorrhage *after* the expulsion of the *placenta* were forty-three ; of these, in three, the duration of labour was not entered.

Of the forty cases accurately stated ; *thirty-four* were delivered within *twelve* hours ; thirty of these thirty-four were delivered within *six* hours ; twenty-five of the thirty within *four* hours ; and eighteen of the twenty-five within *two* hours. Of the total forty cases, there were *only* FOUR above twenty-four hours in labour.

These are *facts* which our observation had so thoroughly placed before our eyes, previous to making an absolute calculation, the circumstance of Professor Hamilton advancing the contrary, as the *rock* upon which the chief corner of his building was to rest, filled us with wonder ; and thus the necessity and wisdom of *searching* for a sure foundation becomes glaring. We are sure it is not Dr. Hamilton's practice to put his candle under a bushel, and therefore hope our illumination will not be too bright for his eyes. Is it possible that statements could be opposed more directly to *facts*, than those advanced by Dr. Hamilton, and given by him as the origin, cause, and basis of his thoughts and deviations from the ordinary mode of practice.

How, it may be asked, is it possible, that Professor Hamilton can appeal to the facts recorded by Dr. Collins, in proof of the correctness of his " Practical Precepts ?" This he *attempts* by ingeniously *culling* a few cases out of the *multitude*, with a tact which no one except Professor Hamilton has yet displayed, giving the most *distorted* view of the facts and practice recorded : no enviable talent most will admit. I hope, however, the *salutary* cry raised by the " seriously offended" stu-

dents, will, like the shore bell in the mist, serve to guide the unwary to the right haven.

I cannot avoid quoting the following passages from the London British and Foreign Medical Review, one of the best Journals of the day, and one that may certainly be considered an impartial witness.—No. 8, Oct. 1837.

“ We most perfectly agree with Dr. Collins, that the interference recommended by Dr. Hamilton must, if *generally* adopted, be fraught with danger to the patient. It is decidedly opposed by every teacher and writer of midwifery, with the single exception of Mr. Burns, and we have always thought it our imperative duty to guard pupils against such practice. We oppose as strongly as the courteous freedom of criticism will permit us, the rules laid down by Mr. Burns, and countenanced by Dr. Hamilton, for the guidance of young practitioners, whose impatience in the practice of midwifery, to cut short their patient's labour, that they may end their own, it is always so essentially necessary to guard against.”

“ We agree entirely with Dr. Collins, that the number of hours a patient has been in labour ought to govern our practice much less, if it should have any weight at all, than the previous history of the patient, and her actual state and condition at the time of labour.”

“ In justice, too, to Dr. Collins, we must add, that Dr. Hamilton has not shewn sufficient attention to his (Dr. Collins's) record of cases, as they occurred in the Dublin Lying-in Hospital, or he would not have referred to them in support of his opinions.” “ We do not perceive that Dr. Hamilton's reply* at all blunts the force of Dr. Collins's criticism, and *still less* does it appear to us to shew that Dr. Collins has either *mistaken* or *misstated* his opinions.” Again “ In reference to the remarks which are made at page 99,* *et seq.*, upon the practice in the Dublin Lying-in Hospital, under ‘ the very able superintend-

* See Med. Gazette, June 13, 1837.

† Practical Observations.

ance of Dr. Collins and Dr. Kennedy,' we must observe that Dr. Collins *proves*, in his 'Observations,'* p. 46, that Dr. Hamilton has not read the work he criticises with sufficient attention, and that, consequently, many of his strictures (to use a mild term) are incorrect and unmerited."

Stronger language, nor from a more impartial source, could not be adduced, than that referred to, in condemnation both of Dr. Hamilton's practice and criticisms.

I would next particularly refer to an article on the Management of the first Stage of Labour, by Dr. Edward W. Murphy, of Dublin,† which is one of the most instructive with which I am acquainted on this subject. It is written with much attention, and careful examination into the most extensive records; and *facts* alone are stated in support of what he has advanced.

In conclusion he states: "Having thus sought for *proof* to determine the validity of the doctrine so warmly advocated by Drs. Hamilton and Burns, as well as the propriety of the means by which they have proposed to effect it; the conclusion to which I have been led appears clearly against both.

"1st. *No proof* is given, neither do the records of the largest hospitals in Europe, nor their practice, establish, that the prolongation of the first stage of labour beyond fourteen hours, so impairs the vigour of the uterus as to become dangerous to the mother or child.

"2nd. That in cases where the pains are continuing often and decided, while the os tincæ is lax, dilatable, and thin, the uterus hardly ever fails, unless from some obstruction in the second stage, in expelling the child with safety to both; and therefore, that the practice of hurrying on the first stage of labour is totally unnecessary.

"3rd. That considering the structure of the os tincæ, how readily a derangement in the order of labour may be produced,

* Dublin Journal, March, 1837. † See Dublin Journal, May, 1837.

and its liability to be inflamed from irritation, such a practice might become absolutely mischievous."

Such are Dr. Murphy's deductions, and his opportunities for personal observation, as assistant in the Dublin Lying-in Hospital, render him highly qualified for the task he undertook.

I need scarcely say I agree with Dr. Murphy's deductions to the very letter, and in my opinion his paper is most interesting and useful.

In the second Medical Report of the Western Lying-in Hospital and Dispensary of Dublin, just published in the preceding number of this Journal by Dr. Churchill, he has given some most instructive *tables*, marking in twenty-one cases of labour, of *thirty-six* hours' duration and *upwards*, taken indiscriminately, the duration of each stage and the issue of the labour to the mother and child. Dr. Churchill gives the following practical conclusions:

"Of the twenty-one cases of labour, varying in duration from *thirty-six* to *ninety-six* hours, in only *four* did the second stage amount to more than four hours, whilst in *eleven* it was concluded in *one* hour. Neither did the duration of the second stage increase in proportion to the prolongation of the whole labour, for of the three cases of ninety-six hours each, in only *one* did the second stage exceed *three* hours.

"Further, it will be perceived at once that these tables have a *peculiar* bearing upon the interesting controversy lately carried on between Dr. Hamilton, of Edinburgh, and Dr. Collins, of Dublin.

"So far as the series of *facts* I have just presented extend, they are in *direct opposition* to the expressed opinions of Professor Hamilton; for a prolonged *first stage* neither rendered 'the powers of the uterus inadequate to expel the infant with safety to its life, or the future well-being of the patient,' nor disposed the 'uterus to contract irregularly, so as to occasion retention of the placenta,' nor too feebly 'to prevent fatal hemorrhage;' nor lastly, did it give rise to 'febrile or inflam-

matory affections of a most dangerous nature.* ‘ For *firstly*, all the children were expelled alive, and continued to live, except two ; one of which was premature, (six months,) and the other presented with the funis, and whose deaths were consequently not attributable to the protraction of the labour. *Secondly*, neither flooding, retention of placenta, fever, nor inflammation, happened in any case ; on the contrary, every one of the cases recovered as well as after an ordinary labour of twelve hours’ duration.”

Could *facts* more graphically illustrate the justness and propriety of condemning Professor Hamilton’s *hasty innovations*, than those I have now accumulated from sources I have no doubt *satisfactory*.

It would be interesting for the reader to compare the multiplied *truths*, and the vast, perhaps I should say unexampled accumulation of *results*, I have submitted in this communication ; with the artful and obviously incorrect *culling* of cases with which I have charged Dr. Hamilton, and which the statements I have above recorded *prove*. The following examples exhibit his talent in this respect, and are *fair* samples of his selections ; and it is strange if they do not make a *lasting* impression on the mind of the reader. They are brought *forward* by Dr. Hamilton, as he “ flatters himself,” as furnishing *the most decided proofs* of the *injurious consequences* of the *indefinite protraction* of the first stage of labour, and of the *VALIDITY* of the arguments upon which he *originally founded* “ the rule of *limiting the duration* of the first stage to *twelve or fourteen hours*.”

I shall first record the Professor’s *doctrine*, and then the cases *selected* by him from my work.

“ *Secondly*. After the birth of the infant, the uterus may contract *irregularly*, so as to occasion the retention of the placenta.”

* See these statements in Dr. Hamilton’s letter as the basis of his “ innovations,” p. 203, May Number.

In proof of the *soundness* of his doctrine he refers to my Practical Treatise, page 142, Case No. 110. “ This woman was sixty hours in labour ; shortly after the birth of the child there was considerable hæmorrhage, with hour-glass contraction, which was treated accordingly. The child died thirty hours after birth.”

Is it not strange to see such deceitful quotations made. Had I not *proved* (p. 407) that the *case given* is the ONLY SINGLE INSTANCE out of 16,414 deliveries, in which the uterus acted *irregularly* where the duration of labour exceeded *twenty-four* hours, I apprehend my professional brethren *might* doubt the fact, the circumstance is so *astounding*. Nay more, of the *total* seventeen cases recorded by me, *fourteen* were delivered within *twelve* hours.

These facts were *staring* Professor Hamilton in the face, in the *very minute* details and *tables* given ; yet he cites the case as establishing HIS *doctrine*, when it *absolutely demonstrates* the almost TOTAL IMPROBABILITY of such an occurrence *ever being met with*.

I shall next shew his *selections* in proof of the “ validity” of the *third* head of his “ innovations” and “ deviations,” and indeed I regret the necessity of such exposures. I first state his *doctrine*.

“ *Thirdly*. After the expulsion of the placenta, the contractions of the uterus may be too feeble to prevent *alarming hæmorrhage*.”

Cases chosen in *proof*, page 169, Case 130. “ In this case the labour lasted fifty hours, the foetal heart having ceased to pulsate, and the head having made no progress for several hours, the mother’s pulse being 120, the head was lessened, and delivery effected by the crotchet. The placenta was thrown off in half an hour, followed immediately by considerable hæmorrhage. Increased pressure was made, and cold applied ; but in five minutes it returned to a serious extent, when the hand was passed, and the uterus being emptied of its contents, contracted

well, and the discharge ceased; she got then thirty drops of tincture of opium to procure rest."

Page 169, Case No. 129. "This woman was sixty hours in labour; immediately after delivery, severe hæmorrhage ensued, which was checked by the introduction of the hand; after which she got forty drops of tincture of opium."

Will it be believed, that these *two* cases, with *two* others, were the ONLY INSTANCES met with in the 16,414 cases where hæmorrhage occurred after the expulsion of the placenta, where the labour exceeded *twenty-four* hours. But what is still more REMARKABLE; of *forty* cases of this description of hæmorrhage recorded by me, THIRTY-FOUR were delivered within *twelve* hours; *thirty* of these thirty-four within *six*-hours; *twenty-five* of the thirty, within *four* hours; and *eighteen* of the twenty-five, within TWO HOURS.

I could not have conceived any intelligent individual making the *rash* references I have shewn; but to adduce such as *the recorded support* of the *validity of new doctrines and new practice* is beyond *all comprehension*. That Professor Hamilton has done so is a *fact*, however startling. I do hope Dr. Hamilton will *candidly* state his *error*, as he has done with respect to the reference given in his *Practical Observations* to the cases of laborious labour published by me; regarding which I found it requisite to make the following comment in my previous communication in March, 1837, "That Professor Hamilton's 'positive decision' respecting the cases here noticed, originated in his *own imagination*, and not from the facts detailed, is accurately demonstrated by himself."*

I come next to notice Professor Hamilton's deductions as to the treatment of laborious labours in the previous number of

*"In illustration of this important precept, I referred, in the second part of my *Practical Observations*, p. 100, to several of Dr. Collins's recorded cases; and I find that, in so doing, I had committed a gross error, for which the most ample apology is due to Dr. Collins."—See May Number of Journal.

this Journal ; and if the practical precepts inculcated by him for nearly half a century, set forth similar *reasoning* and *doctrines*, it is *fortunate* the Professor has to complain that the eminent practitioners of London, Paris, and Dublin have not adopted *his* opinions. As such statements might probably mislead the junior members of the profession, who have not had any opportunity of examining for themselves, I shall endeavour shortly to put them on their guard, and this will be done *most effectually* by laying before them the *results* consequent upon the practice advocated by Dr. H., *as compared* with that which I have had the pleasure to submit from the Dublin Lying-in Hospital. I have to *complain loudly* and *feelingly* of Dr. H. withholding this *all important* practical TEST in *all* and *every* of his voluminous and copious extracts, thus blinding the unwary and stifling the truth.

Professor Hamilton, after *selecting* seven cases from amongst the *worst laborious labours* met with during my residence in the hospital, states, “ Instead of commenting on the above cases, I shall quote the words of Dr. Collins himself:—

“ I know no case where the advantage derived from the use of the stethoscope is more fully demonstrated, than in the information it enables us to arrive at with regard to the life or death of the foetus in the progress of tedious or difficult labours.

“ Heretofore we were in a great measure ignorant of the time at which death took place ; and the practitioner, imagining the child alive, from want of satisfactory evidence of its death, delayed interfering until his patient was in the greatest possible danger ; whereas, had he been assured the child was dead, he would have delivered her before life became actually hazarded, and thus prevented her not only enduring for hours, but even days in some instances, the most torturing pain ; the result of which continued suffering was not unfrequently death, or what was, perhaps, worse than death, extensive sloughing of the urethra, or of the recto-vaginal septum, establishing a

communication between these two cavities, reducing the unfortunate sufferer to a state of extreme misery."—*Practical Treatise*, p. 18.

In reference to the above quotation from my work, Dr. Hamilton observes: "Notwithstanding the excellent maxim thus quoted, it is evident that the symptoms indicating the approach of danger had been totally overlooked in the above cases, and they establish, beyond a possibility of contradiction, my first proposition; for, in the cases alluded to, inattention to that rule proved *fatal* to the *mother** and to the child."

Secondly, "Dr. Collins states, that in 16,414 women delivered in the Dublin Lying-in Hospital during his Mastership, there were twenty-four forceps cases; that four of the women died, and that eight of the infants were still born; in plain language, that one-sixth of the women and one-third of the infants died; and yet the great object in the application of the forceps is to save both mother and child. Let this result be *compared* with that of the practice adopted in the Hospice de la Maternité of Paris.

"According to Baudelocque, during nine years, ending 1806, there were 12,751 deliveries in the Hospice de la Maternité. Out of these, the forceps cases were thirty-seven, being one in $344\frac{2}{3}$; and the crotchet cases were nine, being one in $1416\frac{2}{3}$.

"Madame La Chapelle has given an account of the result of the practice in the same hospital, preceding the 31st December 1811. She states, that in 15,652 deliveries in that hospital, there were ninety-three forceps cases, and seventy-two of the infants born alive. In the details given by Madame La Chapelle, she acknowledges only the death of thirteen women,

* Of the seven cases referred to, *four* recovered without a bad symptom; although *culled* from the *worst* cases of *laborious labours*, and are recorded by me in accounting for the children being *still born*. *Fifteen* women *only* died, who were delivered by the crotchet in difficult labour, out of 16,414. *Fourteen* of the fifteen were delivered of *first* children; all were *males*.

which is one-seventh instead of one-sixth, which occurred in the Dublin Lying-in Hospital.

“But this is not the only fact which requires the most serious attention. In the Dublin Lying-in Hospital, according to Dr. Collins's records, the forceps were employed only once in 608 cases ; whereas in the great hospital at Paris, according to Baudelocque, it was used once in $344\frac{2}{3}$; and according to Madame La Chapelle, in the same hospital, once in 165. In the Edinburgh General Lying-in Hospital, during a very limited period, the forceps were used one in 109.

“On the other hand, in the Dublin Lying-in Hospital, the crotchet was used in laborious labours, according to Doctor Collins, once in 210 cases. Whereas in the Hospice de la Maternité, according to Baudelocque, crotchet cases bore the proportion of one in 1416 ; and according to Madame la Chapelle, one in 1908 ; while in the Edinburgh Lying-in Hospital, the proportion was one in 481. The reader will now find no difficulty in deciding to whose practice the accusation (Dr. Collins, Dublin Journal, p. 30,) of ‘cruelly destroying the child’ is applicable.”

Such are Professor Hamilton's *deductions*, and in “plain language,” I verily believe in the whole history of letter writing a less trustworthy or more unsound chain of reasoning could not be produced, nor any more designed to *fetter* the youthful mind in seeking after truth. Are not, I would ask, the *arguments* brought before us positively *mischievous* in the extreme, when “volunteered” to the *junior* members of the profession, by holding up for their *example* and *imitation*, the utility and advantage of *numerous* deliveries by the forceps, as in the French and Edinburgh Hospitals, in comparison with the Dublin Lying-in Hospital.

What Professor Hamilton's object was in *withholding* the REAL RESULTS of the practice in the institutions alluded to, he best knows himself ; but to my judgment, his *comparisons* are palpably calculated to *conceal* what is worthy of being known, and to

propagate error. That this denunciation by me is fully merited I shall now briefly explain.

Dr. Hamilton, after stating the total number of deliveries in the Dublin Lying-in Hospital under my care, and then the total number in the Hospice de la Maternité under Baudelocque and Madame La Chapelle, gives the relative proportion in which the forceps and crotchet were used, as I have recorded in his own words above. It will be observed, his comment with respect to Madame La Chapelle's forceps cases is, "*she acknowledges only the death of thirteen women,*" (*non credo*, says the Professor!) "*which is one-seventh instead of one-sixth, which occurred in the Dublin Lying-in Hospital.*"

The *real comparative mortality*, which is *scrupulously* concealed in this and *every other* instance by Professor Hamilton, and which was placed before his eyes in p. 343 of my Practical Treatise, is as follows :

Baudelocque states, that out of 17,308 women delivered in the Lying-in Hospital at Paris, *seven hundred* died, which is nearly in the proportion of *one* in every twenty-four. This is a *very excessive mortality*. It does not, however, differ much from the average mortality of *that* institution at the present day.

In some years the mortality in that establishment falls little short of that occurring in most pestilential diseases. As an example, in 1829, the mortality was *one* in every twelve or thirteen women delivered, the number of deaths being 255 in 3074 deliveries; thus in ONE YEAR, a *greater* number of deaths by *nearly a half* occurred, than were met with during my SEVEN YEARS' residence in the Dublin Lying-in Hospital, where 16,414 women were delivered.

Baudelocque reports 700 deaths in 17,308 deliveries, whereas, in 16,414, we had 164.

Is it possible a greater *concealment* of *important facts* could be exhibited than is here detailed. I am of opinion, few will, after this exposure, hesitate in declaring, from Professor Hamilton's *comparisons* of the forceps and crotchet, that his deduc-

tions are most *unjustifiable* and *incorrect*, as well as inconsistent with sound practical instruction, and highly calculated to propagate *mischievous intermeddling*.

Now, for the Professor's *own* hospital, the Edinburgh. In the following statement, taken from Dr. Hamilton's third letter to the Editor of the London Medical Gazette, there is much of concealment.

“It will, no doubt, surprise Dr. Collins and the gentlemen connected with the great establishment in Dublin, when I state, that by a report presented to the managers of the Edinburgh General Lying-in Hospital, and circulated under the authority of the Right Hon. the Lord Provost of this city, dated the 21st January, 1837, it appears that 15,936 women had been delivered previous to the 1st of October, 1836, and that the whole expenditure, (not the annual,) including the purchase of the buildings and area, furnishing the same, &c., amounted to the very small sum of £10,214 13s. 8d.”

Would not the *unsuspecting* reader from this extract believe, that the total 15,936 women were delivered *in* the Edinburgh Hospital; whereas the fact is *far otherwise*, as by a statement printed in 1834, (I have not that to 1836,) the deliveries *within the walls* from 1793 to the former year amounted only to 5,198.

This embraces a period of 41 years, which shews the *annual average* of deliveries *in* the hospital to be reduced to the *very small* number of about 126.

I find by an Essay published in 1823, by the late and much regretted Dr. Mackintosh, of Edinburgh, that Dr. Hamilton “acknowledges”* *seven* women *died* in the Edinburgh Lying-in Hospital in the year 1821; and *five* *died* in the year 1822; making a total of *twelve* in the two years, and that according to Professor Hamilton, *not one case* of puerperal fever occurred in the above period. If we take the average number delivered,

* I use the word acknowledge, from Dr. Mackintosh's statement, as he asserts *nine* women died in 1821, and offers any pledge as to the truth of what he stated.

126, and multiply this by two, it gives us the total number of deliveries in 1821 and 1822, viz., 252, and if we divide this number by 12, the *total deaths*, it proves the *mortality* in the Edinburgh Hospital to be *one in twenty-one*; which is even *more frightful* than the Paris Hospital. By Dr. Mackintosh's calculation, the mortality would be *one in eighteen*. The number I have given as delivered in 1821 and 1822, may be slightly incorrect, owing perhaps to a greater proportional number being delivered in some years than others, but the difference can affect the calculation to no material extent.

I ask Dr. Hamilton, and my *junior brethren* in the profession, to COMPARE this *excessive mortality*, with *one in one-hundred* out of 16,414 deliveries, which I have had the happiness to report as the result in the Dublin Lying-in Hospital during the *entire* period of my residence. But, still *more*, to compare the result of *one death* in TWENTY-ONE, with the result of *one death* in ONE HUNDRED and EIGHTY-SIX, as in the Dublin Lying-in Hospital during the last *four years* of my residence, *after* the disappearance of *puerperal fever*; and this out of the vast number of *ten thousand seven hundred and eighty-five* deliveries.

The *difference* in practice is truly remarkable, and the familiar comparison of *light* with *darkness* very *appropriate*. He that runs may read.

Why is it that I have been thus compelled to *seek* and *fish* for information respecting the mortality in the Edinburgh Hospital, which is a public charity, supported by public subscription? In my observations in this Journal in March 1837, I thought I had sufficiently called Professor Hamilton's attention to the necessity of making a proper report in the following words:

"I cannot help stating the additional pleasure and *real satisfaction* I should have experienced, if Professor Hamilton had embodied with his highly interesting work, a brief report of the Edinburgh Lying-in Hospital, of which it is so well known

he has had the medical charge for *nearly half a century*. This indeed would be of *great value*, as a report of the cases taken during the *progress* of the patient's illness, *marking* the *cause* of *interference*, in those cases where a *deviation* from the *ordinary treatment* was had recourse to, would be *highly instructive*; at the same time *making us acquainted* with the RESULT in *all* cases to both *mother* and *child*. Without the *latter* no *correct opinion* can be formed of the ELIGIBILITY of any line of practice *recommended*. I *anxiously hope* for this information with respect to the Edinburgh Hospital, even for *half* the period mentioned, so as to *enable us*, as stated by Doctor Hamilton, "*to contrast the result of his practice* with the recorded evidence of the protraction of labour in London, Paris, and Dublin." In reply to the above, Doctor Hamilton in his third letter in the London Medical Gazette, states "it is not in my *power to comply* with Doctor Collins's wish." He accounts for this by stating, that previous to the 4th of January, 1823, many of the early records were abstracted, but that *since* that period the directors of the institution, at his suggestion, ordered the details of the cases should in future be inserted in "*ponderous ledgers*." This, it may be observed, embraces no shorter a period than *fifteen years*, in which the records of the hospital are *perfect*: and all this valuable information is *withheld*, because the entire *half century* cannot be included. Our Report of the Dublin Lying-in Hospital only extended over *seven years*. It appears strange that Professor Hamilton can report the *total number* of cases of rupture of the uterus in the Edinburgh Lying-in Hospital since its *foundation*,* and yet he leaves us in the *dark* (after the *example* of Madame Boivin and Madame La Chapelle)

* See first letter Medical Gazette, p. 17. It is well known that Dr. Hamilton has held the Professor's Chair of Midwifery in the Edinburgh University since the first establishment of the Edinburgh Hospital; and I have no doubt, the notes of his lectures contain an abstract of all the important occurrences.

as to the *total mortality* to the *mother* and *child*. I still hope Dr. Hamilton will re-consider the subject, and submit to the profession a report of that character and minuteness he is competent to; and thus confer a lasting benefit upon his brethren, and at the same time requite the confidence placed in him by the public, in retaining him so long in charge of the charity.

The last point in Professor Hamilton's letter, I shall at present shortly direct attention to, is one of the *utmost* practical utility, in my opinion, in the management of laborious labours; viz. the ascertaining with accuracy the *life* or *death* of the child by the stethoscope. I firmly declare it as my belief that it is one of the greatest improvements ever made in the practice of midwifery; notwithstanding the Professor's sneeringly nominating it the *new method*! I could not have believed that Dr. Hamilton was so totally unacquainted with the advantages to be derived from mediate auscultation, as his criticisms prove him to be. Dr. Hamilton remarks, "By what extraordinary process of reasoning any practitioner, with the conviction that there is such an obstacle to the delivery, that a living infant cannot be born, should delay the necessary relief to the woman, till by means of the stethoscope the death of the infant should be ascertained, is to me quite incomprehensible.

"And yet the following cases shew, that this principle was carried still further in the Dublin Lying-in Hospital, for it is admitted, that notwithstanding the *alarming symptoms*, the sufferings of the patient were allowed to continue for *hours* after the death of the infant was believed to be unequivocally ascertained by means of the *new method*, on the important utility of which Doctor Collins has passed so high an eulogium."

Such are Professor Hamilton's enlightened conclusions. In justification of the high sounding language I have quoted, he has *selected nine cases* in which it was deemed advisable to effect delivery by the crotchet, the child's *death* having been

ascertained by the stethoscope *some hours* previous. What, I ask Doctor Hamilton, was the result even of the *nine* cases *chosen*? I will answer; *all but one perfectly recovered*. What possible stronger evidence could be adduced than is afforded by these very cases, in several of which there was great difficulty, yet by the child's death having been satisfactorily ascertained, and delivery effected shortly afterwards, without the most remotely disagreeable feeling as to the destruction of the child, *eight* of the *nine* escaped injury. It appears remarkable, in Doctor Hamilton's judgment, that the patient should be allowed to remain undelivered for *some hours* after the child's death is ascertained. Here Doctor Hamilton *as remarkably* displays his want of acquaintance with the application of the stethoscope, as *he should have known* that it is totally contrary to every rule of practice, to deliver the patient with the crotchet when the foetal heart's action *first* becomes inaudible. It is only after frequent examinations, with some interval between each, that this is ever resorted to, except where the mother's life is in *very imminent danger* indeed. Is it probable the *nine* cases referred to would have terminated so favourably, if we had not had the means of *detecting* the child's death, which enabled us conscientiously to effect delivery before the mother suffered any serious injury. Doctor Hamilton states, "*he cannot imagine a case of laborious labour, which had been much protracted, where the knowledge of the state of the infant can be necessary to regulate the practice.*" This indeed is an *alarming* assertion, and never could have been made by a practitioner who diligently applied the stethoscope in such cases. Does Doctor Hamilton mean to convey by this expression that he would destroy a *living child* where the mother's life was not in *danger*? If so, we solemnly protest against so revolting a doctrine.

Again, does Doctor Hamilton believe, that *without* the use of the stethoscope, he could discover the time at which the

child's *death* occurs in laborious and difficult labours. I am of opinion few will be so hardy as to state so.

Is it not therefore a matter of incalculable advantage to the practitioner to have the means of judging on this subject; as from the natural dread every man must feel to open the head of a *possibly* living child, he would otherwise refrain from interfering, until the sufferings of the mother had been so great as to place her in the utmost jeopardy.

I have no hesitation in stating, that had not the stethoscope been attentively used in the Dublin Lying-in Hospital, during almost the entire period of my residence, we could not have reported so *vast* and *singularly striking* a difference in the *mortality* with other similar institutions.

When we reflect, that according to the calculations I have given from the Edinburgh Hospital, no less than NINE women *died* for every ONE that *died* in the Dublin Lying-in Hospital, after the disappearance of puerperal fever; it is evident Professor Hamilton's DEVIATIONS from the ordinary mode has been a "*most unfortunate practice.*"

Professor Hamilton states, in alluding to a case of protracted labour, which he records, p. 204, "I should have applied the *forceps* and *extracted* the *infant.*" "In the above instance, therefore, it must appear to any candid inquirer after *truth*, that both mother and child *were victims to the practice adopted*: the *cause* of protraction was evidently the interception of a *band* of the *cervix uteri* between the head of the infant and the pubes."

This bombastical and presumptuous language is in my opinion truly unworthy in any individual, and still more so from an aged Professor, who should certainly not instruct his pupils in scurrility; which every well regulated mind invariably treats with the *utmost contempt*. My statement regarding the delivery of this patient is, *that after the head was lessened, ALMOST EVERY BONE WAS REMOVED before it could be delivered; and even after it was brought down, MUCH EXERTION* was re-

quired to free the shoulders and body. The child (a *first*) was *large*, and the body somewhat *distended* with *air*. Would any "*inquirer after truth*" have made the unfounded and unprofessional remarks above, with such a detail before his eyes. I cannot avoid stating, that if commentators *similar* to Professor Hamilton were *numerous*, who would sit down to *cull* and *distort* cases in support of their own doctrines, and at the same time scrupulously conceal the *really valuable and obvious results*, to the *disadvantage* of the writer, as I unhesitatingly submit I have demonstrated Doctor Hamilton to have done in every instance noticed in this communication ; it is my opinion, that physicians attached to hospitals would then be *quite justified* in reporting the results once in "half a century." Fortunately, however, they are *rare*, as I have met with *no other* to act so unfairly ; and until Professor Hamilton proves to his brethren in the profession, by a minute detail of the cases under his care in the Edinburgh Hospital, somewhat similar to what I have done from the Dublin Hospital, that his practice has been attended, not only with *equal*, but *happier* success, he should be *ashamed* to make such gross remarks as have sometimes escaped him : and his criticisms upon others, so long as he *hides* his *own practice* in "ponderous legers," must prove *abortive*.

It is surely not to *select*, and comment *unfairly*, on a *few* cases, that a work containing a report of *sixteen thousand six hundred and fifty-four births* should be studied. As I before stated, it never should have been published, if the author had not widely different and more extended ideas of its utility. It would ill become me to say much respecting the manner in which the work has been received ; if, however, the opinions already so *freely* expressed by the professional public and reviewers be a test of its merits and utility, I need feel very little anxiety as to the opinions of any one individual on the subject.

In concluding these observations, I beg to assure Doctor Hamilton, that had he candidly and fairly stated the *results* of

the practice of the Dublin Lying-in Hospital, simultaneously with his comments upon the several points of practice wherein we differ, I should have felt much pleasure in discussing the subjects with him calmly and deliberately ; but when this *important* information is in every possible way *stifled*, and cases (*from the multitude*) ingeniously *selected* in *proof* of doctrines I have clearly shewn they absolutely DISPROVE ; I feel that I so little deserve this treatment, and that there is such a *total* abandonment of generosity in the course adopted, that I have been tempted to *reprint* occasionally some of the Professor's own language, which I hope he will agree with me in thinking, when it is applied to *himself*, unworthy of the author, and in every respect calculated to prohibit that *friendly intercourse* and *freedom of opinion* so truly advantageous to the spread of knowledge in the profession.

ART. XIX.—*Researches on the Occurrence of Typhus in the Manufacturing Cities of Great Britain, with some Observations on the Nature of its Enteric Complication.* By JULIUS STABEROH, M. D., Berlin, &c.

[Read before the King and Queen's College of Physicians in Ireland.]

GENTLEMEN,—In addressing these observations to you, I must say, that although I felt grateful for the unmerited mention of my name by the reviewer in the Dublin Journal for January, 1838,* I found my condemnation also previously pronounced in the correction of Doctor Lombard's somewhat rashly formed opinion.

It is now five years since I commenced the careful study of continued fever ; the last six months have been spent in Great Britain and Ireland, and I am every day more convinced, that a foreigner, unless he were to spend several years in studying the disease in different localities, would never acquire accurate

* See the Review of Dr. Cowan's Statistics of Fever.

knowledge on the subject; and that it is your province by collecting and comparing the different phases of the affection, to teach us what is essential, and what is accidental in its appearance. During my stay in Dublin, I met for the last three months of 1837 with a type of continued fever, which varied very little, generally speaking; although I saw in the two last weeks of December a less marked eruption on the patients, and especially a lesser liability of the spots to assume that state, which I may be permitted to call the ecchymotic, when they no longer disappear on pressure. It would be ridiculous to offer a farther account of it to you, under whose eyes I studied it, but as I think that every man who has been so kindly received, as I had the happiness to have been by you, is called upon to refute any erroneous assertion which has been advanced against your country, I take the liberty of laying this paper before you, in which I will endeavour to give some proofs of the opinion of the respected reviewer; *that Irishmen neither bring typhous fever over to Great Britain, nor keep it up in the heart of the British cities.* With this intention I will relate what I observed since the 1st January, 1838, during which time I studied the typhous fever in Glasgow.

In the first week of January the eruption was very well marked in the greater number of cases;* until the 15th January inclu-

* I cannot dwell upon the prevalence of eruption statistically, as in this list I marked every case where it was to be seen, paying no attention to its degree.—My friend Dr. Anderson gratified me with the promise, that in future he would mark in his list, three degrees or forms of eruption; as first, measles-like or papular spots, the eruption described by Hildenbrand, Peebles, Perry, and I may add, observed commonly during my attendance on the Dublin Hospitals in October, November, and the first week of December, 1837; as second, eruption but little elevated, yet well marked; third, a few pale spots, representing what is described by the French authors as eruption typhoide, tâche typhoide. That this distinction will facilitate the decision of the question whether there be any essential difference between dothineria or typhus abdominalis, and exanthematic typhus, or continued fever without abdominal complication, I feel convinced.

sive, 125 cases with the eruption, and 27 without it, have been sent to the fever hospital; in one case the eruption was doubtful. The affection of the chest was so prevalent, that nothing but coughing was to be heard, and the cough had very often a barking sound; nevertheless, the pneumonic complication was rare; in the *post mortem* examinations of those who died at this period, (although one only out of thirteen was not allowed to be inspected,) we found typhoid pneumonia of the lower lobes once; and only twice, considerable congestion of the postero-inferior part of the lungs; this circumstance is remarkable, as in the year 1836, while Doctor Cowan attended the hospital, typhoid pneumonia was not uncommon at the same season of the year. At the end of the above mentioned period pain in the epigastrium, which was not uncommon before, became very frequent, and was no longer referrible solely to sympathy with the bronchitic affection; it was subsequently attended by tenderness of the hypogastria, especially the ileo-cæcal region, and the cough became every day less prevalent; about the end of the third week of January we heard scarcely any cough, but the hypogastric tenderness was remarkably frequent.

It was during this week that I had the pleasure of meeting my friend Doctor Evans, of the Madras Service, from whom, when he favoured me with his company in attending the fever-hospital, I frequently inquired, in pointing out certain cases to him, whether he would perceive any difference in the character of the fever between these cases, and those which generally occur in the Parisian hospitals, where we had observed them for several months together: or whether Professor Louis, were he describing those cases in his account of the *fièvre typhoïde*, would find any difference worthy of notice, from those which he has already published; and I was always satisfied to hear my friend, when talking of abdominal typhus, call it the form described by Louis.

The exanthematous eruption was marked in 117 cases; until

the 28th January inclusive, 19 patients did not show it. I was prevented by illness from attending the hospital regularly for the following days, until the 1st February, although not entirely confined to bed ; I tried to visit it occasionally, but I was told by my friend Doctor Anderson, the experienced physician of the hospital, that cough became prevalent again, and the abdominal affection was much less marked.

In the *post mortem* inspections from the 16th until the 25th January inclusive, after which day I would no longer assist at them regularly, serous exudation on the surface of the brain was found in three cases ; typhoid pneumonia in three ; also congestion of the lower lobes of the lungs in two ; aphthous ulceration of the stomach in one case ; the Peyerian and solitary glands were marked by swelling in five cases ; in one the typhoid infiltration of these glands was as characteristic as possible.

My respected colleagues will excuse my freedom, if I mention here, what I suppose to be the nature or history of the affection of the Peyerian glands ; as this idea is the result of the examination of about fifty cases of death from abdominal typhus (dothineria) in the general hospital of Vienna, under the eyes of the Prosector of this well-known medical institution. Not to exhaust the patience of my readers ; I think that there is an infiltration of a matter, whose nature and origin I cannot yet determine, not having examined it sufficiently with the microscope ; very probably the result of deranged secretion in the mucous coat of the lower part of the intestine, and particularly in their crypts, which are called Peyerian glands ; but also taking place occasionally in other parts of the intestine, as I saw it repeatedly, and lately in Glasgow, in different parts of the colon.

The inflammation which takes place in the mucous coat of the intestine (gastro-enterite, dothineria, enterite typhoide, &c., &c.) is merely a reaction following upon this infiltration, in the same way as the skin becomes hot, red, in-

flamed over a collection of scrofulous or other purulent matter, in what is called congestive abscess.

When this problematic infiltrated matter is not resorbed by the returning vital power during recovery, it becomes soft like tuberculous deposits, and the inflamed mucous coat on the top of this dothineritis or enteric abscess bursts; but as we see often in furuncles, that the whole of the diseased cellular tissue is not instantly discharged, feculent depositions stop up the opening again, and give to these abscesses the appearance of exulcerated small-pox covered with crusts. (This appearance induced the celebrated German pathologist, Schoenlein, of Zurich, to consider this typhoid infiltration as an intestinal exanthema.) But the softening of the infiltrated matter goes on under these pseudo-crusts, and when all is softened the crusts give way, so that we see the muscular coat of the intestines without any inflammatory reaction, lying bare in many cases. Even now, recovery is possible: the loss of substance is supplied by two different means, either by the production of a pseudo-membrane;—thus we found in patients, who died from other diseases a long time after having had abdominal typhus, in which by comparison with other cases, this process was supposed to have destroyed the mucous coat and the sub-mucous tissue, depressed places, which appeared lined with a smooth and shining membrane;—or by the formation of granulations, which produce a cicatrix like those which we see in the pharynx, the surfaces of which are sometimes very similar to the surrounding healthy mucous membrane. Hence in some patients, who died a long time after a bad abdominal typhus, from other diseases, the loss of substance in the formerly exulcerated parts of the intestine is not so apparent as in the first mentioned case.

The case, number four, taken from a case in 1836, who died a good while after typhus, from another disease, will show both kinds of the curative processes.

The casts in wax which I take the liberty of laying before you will supply the deficiency of this description, and explain bet-

ter my view; the first shows the typhoid infiltration in the Peyerian glands near the valvula cœcalis Bauhini. We see that in the glands, which appear thickened and infiltrated, no vessels are to be seen, but that around them a circle of vessels is formed, as distinctly as it is to be seen in Kaltenbrunner's plates of inflammation; and that also vessels are ramified in the sub-mucous tissue, next to the muscular coat, (as is seen best with a lens in the cut through the mucous coat in the midst of the glands.) Dr. Paterson took this cast from the preparation I brought to him directly after the inspection, with the accuracy and dexterity for which he is distinguished.

I shewed the preparation to Dr. Anderson, and to Professor Rainey, and both acknowledged that there were almost no vessels to be seen in the glands themselves, although I had the preparation on a plate of glass, and let them examine it by transmitted light.

I shall give but a short account of the case in which the abdominal complication became marked in the third week of January; while in the first weeks the head was especially affected and Dr. Graves's treatment by tartar emetic and opium did remarkably well, so that we thought the patient out of danger for several days. (It is to be observed, that many abdominal cases were in the hospital at this time.) The patient was a native of Ireland, but long resident in Glasgow: the eruption was prominent and very well marked. The second cast gives a perfect representation of the state of typhoid infiltration, when the most prominent point of the mucous coat, which covers the infiltrated and partly softened matter, is already corroded, and the opening of the abscesses closed by plugs, consisting of softened matter and feculent depositions.

I regret much that I cannot send a third specimen, where the infiltrated matter is entirely softened and discharged. I saw but one such case here, but sudden illness prevented me from calling on Dr. Paterson to take a cast of it. This was a

case in Dr. Cowan's ward, and the patient had a well marked typhoid eruption; he died from peritonitis after perforation of the intestines. (He was sent on account of bronchitis alone to the Royal Infirmary, his case not being considered as one of fever.) We found, on inspecting the abdomen, inflammation and membranous exudation on the peritoneum to a great extent, and in the mucous coat of the colon, both near the valvula Bauhini, and in the lower parts of it several ulcers of the kind above described existed, without any inflammatory reaction surrounding them. On the edges of those pseudo-ulcers, I pointed out to my friends Messrs. Stewart and Anderson, as we examined them with a good lens, a yellowish matter still remaining, which in others, which were not so deep, covered their bases, so that the muscular coat was not yet to be seen bare. In the large and deep ulcers where the perforation had taken place, both the muscular coat and the peritoneum forming the edges of the perforation, appeared injected by a more or less strongly marked circle of vessels. This circumstance explains the way in which the perforation takes place; when, from the great vital depression of the patient no reproductory action occurs: an asthenic inflammation surrounds the deepest part of these corroding ulcers, and then the muscular coat and the peritoneum give way by the congestive inflammation, just as the mucous membrane which covered the infiltrated matter was broken from within by the secondary inflammation. This state is represented by two casts, number 3, taken from a case of abdominal typhus in 1836. There are other abscesses also in the Peyerian glands covered with crusts; but the muscular coat and the peritoneums resist frequently a long time, as fibrous and white tissues are always less liable to inflammation in typhous fever.

I may be wrong or not in this explanation of the ulceration in the intestines of typhous fever-patients; but I think that many of the post mortem appearances in this disease, considered as the product of genuine inflammation, are rather the result of

a stagnation and congestion, favoured by the loss of vital power in every organ. Thus, we can easily ascertain that what is called inflammation of the intestinal mucous coat, is certainly most marked, if not existing alone there, in the more depending parts of the intestines, which lie at the time of the inspection in the fossa lumbaris, and fossa pelvis. Then, as we must suppose that in a patient dying of typhous fever, peristaltic motion ceases gradually with the decreasing vital power, we are induced to think that the congestion of those parts of the intestines, which lie in the above mentioned fossa, begins a long time before death, and is increased after it, when the dead body remains in the same position, lying on its back.—Hence, if we mark with pins those parts of the intestine, which are actually the most dependent in the cavum abdominis, before changing their position at all, we find them the most injected.

If we turn the patient immediately after death on his face, we will find, certainly, the parts of the intestines near the abdominal muscles well injected, congested; though the above mentioned parts (which were the most dependent while moribund) appear to be in the same condition, yet, I do not pretend to deny that there is often a catarrhal inflammation of the tube in typhous fever, as bronchitis so frequently exists; but we find this intestinal catarrh in fever cases, when the abdominal complication in what is called true typhous fever is by no means the prevailing affection. This would be an enteritis but not a dothinenteritis.

But the formation of dothinenteric, or enteric abscesses appears to be rather the result of a previous stagnation (congestion?) I may compare it to the typhoid pneumonia, which I think owing to a process quite different from the sthenic inflammation, and kept up, if not produced, by passive congestion in the affected, and very often, in the most dependent parts of the lungs, although this is not the general rule.

The typhoid infiltration exists in some of the Peyerian glands, without any reaction surrounding them, while others (the lower situated ones ?) are already in that state, represented by the first cast, we may ascertain in many cases, but whether it complicates dothineritis is a question afterwards to be decided. I submit these ideas in the hope of being taught by you. But whether I am right or wrong, you will allow me to lay before you hereafter the results of my investigations on the subject.

In the first days of February the most remarkable change in the character of the fever I ever met with took place. I find (in the first week of January) in one of the notes in my journal, that I was astonished to observe the affection of the head, exhibited in the form of delirium, which I saw so frequently in the Dublin fever-wards, much less common in Glasgow than stupor, which sometimes set in very suddenly. But in the week above mentioned, furious delirium appeared in many cases : I may mention that in one ward, (there are six of them, and two for the convalescents, each consisting of three or four apartments,) five patients required the strait waistcoat ; that one escaped the vigilance of the nurse, and broke the window, in trying to get out. In town practice several of my medical friends told me, that the same happened to one of the Professors of the University, who died of an attack of typhous fever, in which the head alone seemed to be affected ; and it is very singular, that one of Doctor Anderson's patients, who had exhibited no delirium during the course of the fever, had during this week a relapse marked by violent delirium and convulsions.

In the second week of February almost no patient in the fever house required constraint ; a morning delirium was very common, so that in going through the wards, we would hear many of the patients muttering constantly, although they answered questions often pretty well, as if awaking from a dream,

and trying to remember in what circumstances they were.— Since the middle of February the fever has become much milder, and has not been marked by any general character.*

I have given a somewhat detailed account of the first six weeks of 1838, as it appears rather improbable *a priori*, that an imported disease should be so much under the influence of epidemic influence, as to exhibit the singular alternation in its character, which I have described, in so very limited a period of time. But although it would not be proper for a foreigner, to tell you what character of the fever has been formerly observed in Glasgow, I wish only to mention, that in the months of February, March, and April, 1836, by far the greater number of fever patients laboured under an abdominal typhus, which is called in France *fièvre typhoïde, dothinenterite*, &c., and that this epidemic would have left no doubt, even to an observer of a few days, as to the nature of the disease, as many patients died with peritonitis, the consequence of perforating ulcers in the Peyerian glands, &c. At all events, at that time, the fever did not come from Ireland, or else it must have been remarkably changed by the passage of fifteen hours, as I am told by many esteemed medical friends in your city, that since 1826 no well marked prevalence of the abdominal complication has characterized the typhous fever of your country.

Nevertheless, as the idea that Irish journeymen could bring over to Scotland the typhous fever of Ireland, carrying the con-

* I have been disappointed in my intention of sending you an exact account of the weather during this period ; I hope to do so as soon as possible. I wish previously to remark, that the weather was mild and occasionally rainy in the first fortnight of January ; cold suddenly set in, in the middle of this month ; but the last five days of it were again rainy ; the first week of February was bright and severely cold.

tagion with them, is not only not wholly rejected by medical men in Dublin, but even frequently adopted in Great Britain, I have tried to ascertain, whether the communication between Belfast, Dublin, and Glasgow is so close, that according to the opinion of one of the most celebrated medical men in Dublin, the circumstance, that Ireland and Scotland are separated by fifteen hours' sail, may be disregarded. I took my lodgings for four weeks on the quay, where the Irish steam-packets arrive, but I never remarked a considerable number of Irishmen coming over, hence my intention to visit their "crowded lodgings" was disappointed, and although there are no accounts kept in the offices of the number of deck passengers who come over, I can assure you from repeated inquiries, that the number was exceedingly small, throughout the whole month of January.

Nevertheless, the number of fever patients treated in the fever house alone during that month, was very considerable,—about 300. When I tell you that 92 of them were Irish, 4 only English, 200 Scotch, the proportion of the Irish seems to be considerable, but I inquired for a long time of all the Irish that went to the hospital, and were able to answer how long they had been in Scotland, and I found that the man who had been the shortest time in Glasgow had come over five weeks before; nor was I ever informed in answer to my questions—that in the houses from which they came, Irishmen, lately arrived, had been received.

If typhous fever was to be carried over at any time, we should expect it in those months, when so many thousand labourers go over to Scotland from Ireland to cut the harvest, therefore I give you the proportion of Irish fever patients treated in the fever hospital by my esteemed friend, Dr. Cowan, in the year 1836, during the months of July, August, and September.

August, 1836.

Admitted	115 males,	with eruption,	of whom	41 Irish.
„	108 females,	„ „ „	27 „*	
„	39 males,	without eruption,	16 „	
„	19 females,	„ „ „	8 „	

September, 1836.

Admitted	116 males,	with eruption,	„	39 „†
„	120 females,	„ „ „	„	39 „
„	24 males,	without eruption,	„	19 „
„	29 females,	„ „ „	„	11 „

But in July, 1836.

Admitted	104 males	with eruption,	„	38 „
„	11 „	without eruption,	„	10 „
„	96 females	with eruption	„	31 „†
„	21 „	without eruption,	„	8 „

Let us compare these numbers with a corresponding table of the months of March and April in 1836, when an abdominal typhus was the most common sort of fever; and certainly the number of labourers going over was very much less.

March, 1836.

Admitted	36 males	with eruption,	of whom	11 Irish.
„	24 „	without „	„	13 „
„	29 females	with „	„	8 „
„	34 „	without „	„	6 „

April, 1836.

Admitted	68 males	with eruption,	„	12 „
„	26 „	without „	„	11 „
„	52 females	with „	„	7 „
„	32 „	without „	„	11 „

* In one eruption doubtful.

† In one doubtful.

‡ In one doubtful.

And I think no person could believe, that the somewhat greater number of Irish fever patients in the first mentioned months, in the harvest time, was by any means in proportion with the enormous troops of labourers coming over at that period.

Nevertheless, it may appear to a superficial observer, that the great difference in the total number of fever-patients during the months of March and April, on the one hand, and July, August, and September on the other, must be derived from the increasing arrival of Irish journeymen; but if he consults the statistics of Dr. Cowan, p. 25, he will find that the number of fever-patients treated in the Hospital increases so gradually, that such an opinion would seem to be quite erroneous. Moreover, I have been told by one of my medical friends in Glasgow, who attended a great number of the lodging-houses, to which the Irish labourers generally resort, that he rather thinks, that those men contract the typhous fever in Glasgow, where they are placed in very unfavourable circumstances, and this, I think, diminishes much the importance of the proportion of Irish fever-patients in the above accounts.

We cannot draw any conclusion from the number of fever-patients, in whom the disease is stated in the journals to have originated from contagion. That many persons are seized with fever in the same house may prove as well, that the same reaction against local inconveniences in those dwellings has taken place in several persons, as the transmission of the disease by contagion. Yet, in giving you the number of Irish and Scotch patients, who are described as attacked by infection in the above mentioned month, I presume, that if you depend upon those assertions, there are rather less cases originating from contagion among the Irish fever-patients.

March, 1836.

Out of 60 male patients, contagion is mentioned for 25 Scotch, 18 Irish.

	(36 Scotch, 24 Irish.)	„	„	„
„	63 female patients, „	„	12	„ 8 „
	(49 Scotch, 14 Irish.)	„	„	„

April, 1836.

Out of 94 malepatients, contagionis mentioned for 32 Scotch, 9 Irish,				
(71 Scotch, 23 Irish.)	„	„	„	„
„ 84 female patients, „	„	35	„	5 „
(66 Scotch, 18 Irish.)	„	„	„	„

July, 1836.

„ 125 male patients, „	„	28	„	12 „
(77 Scotch, 48 Irish.)	„	„	„	„
„ 117 female patients, „	„	17	„	9 „
(76 Scotch, 41 Irish.)	„	„	„	„

August, 1836.

„ 154 male patients, „	„	8	„	5 „
(97 Scotch, 57 Irish.)	„	„	„	„
„ 127 female patients, „	„	19	„	6 „
(91 Scotch, 36 Irish.)	„	„	„	„

September, 1836.

„ 140 male patients, „	„	16	„	6 „
(81 Scotch, 59 Irish.)	„	„	„	„
„ 149 female patients, „	„	15	„	7 „
(99 Scotch, 50 Irish.)	„	„	„	„

It remains then only to ascertain, how far the residence of Irishmen in Glasgow is one of the causes that fever has increased for the last years so much, as Doctor Cowan has shewn in his paper.

Although the number of Irish residing in Glasgow is much inferior to what has been supposed by Doctor Lombard of Geneva, I think I may advance, that the number of Scotchmen who live as labourers in the same circumstances, as by far the greater part of the Irish families do, corresponds at least to the proportion of fever patients given by either nation, and many practitioners told me, on my request, that they never observed a distinct predisposition of the Irish to take typhous

fever. I have also often been told, that the Irish women are generally liked by the landlords, as an industrious and quiet people, a statement which certainly does not speak against what I am anxious to maintain.

Although I cannot dwell upon my own observations, which can only be limited, I trust upon the assertions of several medical men, who attend dispensary practice, that those habits which facilitate prevalence of fever among the lower classes of the people, dirtiness, feeding cattle almost in the rooms, &c., are equally common among the lowest classes of Scotch, especially the Highlanders, who had come into town, to get the means of their subsistence in the manufactories, and amongst the Irish.* But as almost all the Irish who reside in Glasgow belong to the lowest classes, which are in every country the most subjected to fever, the relative number (proportion) of Irish fever patients becomes still less; and I repeat, if we compare the equally poor of each nation, there is never a greater proportion of Irish than of Scotch found to have been seized with fever. This assertion is seconded at least by the fact, that in that part of Glasgow, which is called the Borough, where the Irish population is but very limited, a comparatively much greater number of fever patients was treated by dispensary practice, than in the suburbs, the principal residence of the Irish, as I understand, looking over the bills and accounts given by the district surgeons who attended them. It is true, that in the former a great many lodgings are more crowded than in the latter, but

* It is very remarkable that medical observations of every country shew, that every new population, in any locality whatever, is much liable to exhibit a reaction against those local influences to which they are now subjected. We may call this reaction, continued fever, typhous fever, febris mucosa synochus, fièvre typhoïde; (all but dothinerite.) This assertion was repeated in relation to Paris, still, in the last course of lectures on typhoid diseases given by Professor Andral in the winter of 1836, and finds also its application in the typhous fever of Glasgow; as in the average, those who lived but a short period in town were considerably more frequently seized with fever, than those who had been for a long time in Glasgow.

the same medical gentlemen told me, that those very bad localities in the borough are generally inhabited by the respective Irish population, and if you should admit, that generally speaking the Scotch workmen are more anxious to procure a certain kind of comfort, as far as it is possible amongst those classes, we come to the conclusion, that *cæteris paribus*, the Irish in Glasgow are somewhat less liable to fever than the Scotch. With this induction the statistics of the Glasgow Fever Hospital agree perfectly: because, although the fever patients who are sent to it, belong in the far greater proportion to districts in which the greater number of the Irish reside, I can assure you, that the proportion of Irish patients is for every period almost the same (in the average) as in those months, the statistics of which I have laid before you.*

* I need not refer to the authority of Dr. Cowan's statistics on typhous fever, as its contents are described in the Review of the Dublin Journal of January, 1836; but I may be permitted to add a proposition of Dr. Cowan's account of fever, given in the Western Supplement to Oliver and Boyd's New Edinburgh Almanack, for 1838, page 22. Dr. Cowan says:—

“The Irish, by increasing the number of the labouring class, have thereby contributed to augment the number of deaths, but *most decidedly not* in proportion to their numbers in a greater degree than immigrants of the same class from the Lowlands and Highlands of Scotland, and in calculations of the mortality of the Scotch and Irish inhabitants of Glasgow in Hospital, we must keep in view, that the Irish belong exclusively to the class of community from which the Hospital patients are drawn, while a large proportion of the Scotch are in such circumstances as to place them above such a contingency.” And farther:—“But many causes have been in operation in other towns, some of them resembling Glasgow in the density and constitution of their inhabitants, without producing a similar rate of mortality. Many of the cases of the production and propagation of disease must be ascribed to the habits of our population, to the total want of cleanliness among the lower order of the community, to the absence of ventilation in the more densely peopled districts, and to the accumulation for weeks or months together, of filth of every description in our public and private dunghills, to the overcrowded state of the lodging-houses resorted to by the lowest classes, and many other circumstances unnecessary to mention.”

Dr. Cowan communicated this paper with me, when my own was finished; hence, you must excuse that there seem to be repetitions of his assertions in my account.

All other circumstances are equal, I think ; the mortality is the same in both nations according to Doctor Cowan's statistics, and in the journals I could not find in either nation the prevalence of any peculiar local affection : for the observation made during a few days of my attendance in the fever house, that a greater number of Irish had a well marked exanthema, was ascertained not to be the rule by continued examinations.

I conclude those observations with Doctor Cowan's remark, that although the Irish population is considerably increased in Manchester during the last years, typhous fever is rather less prevalent than it has been many years ago, when but few Irish resided there, and I think I may presume, that if a sufficient investigation was to take place at Liverpool and at Manchester, typhous fever would be found as independent from the arrival and the residence of Irishmen, as it has already been shown to be at Glasgow.

Several of the most celebrated medical men, both in Manchester and Liverpool, with whom I had the honour to become acquainted, during my stay in those cities, are far from supposing such a connexion, and almost none was disposed to adopt that specific difference between typhous fever and gastro-enteritis, or dothineritis, which I am very sorry to say, will perhaps still a long time prevent the medical men, who have not the opportunity of judging both forms, when they prevail epidemically and endemically from taking a general view of continued fever.

ART. XX.—*Cases*. By DAVID H. SCOTT, M.D., M.R.C.S., &c., &c.

PUERPERAL MANIA, BENEFICIAL EFFECTS OF BELLADONNA.

A LADY, the mother of four children, was confined of her last in April of the past year. Since her childhood she has

been constitutionally nervous, and partaking of an hereditary excitability. Her former confinements were not marked by any unusual symptom ; they were easy, rather quick, and her restoration rapid. About two months previous to the full period of gestation, she began to entertain doubts of her safety, and was in the habit of expressing them to her husband, all of whose persuasion was in vain. Her fears gradually increased, and nothing could divest her of the absolute conviction that her life would fall a sacrifice in giving birth to her child. This period of anxiety and alarm soon arrived, her labour was most favourable, and her fears were dissipated, when was personally demonstrated the safety in which she then was after so long and such gloomy forebodings. For a few days she went on recovering, all doing well, until the evening of the seventh, when to his utter astonishment, on returning from the active discharge of his professional duties, her husband found her sitting up in bed, reciting with astonishing rapidity and accuracy several parts of scripture, and hymns, many of which she had learned in her childhood, and which had been apparently forgotten by her, as she was never, on any occasion before, known to repeat them, neither could she when requested to do so. She was quite unmanageable, scarcely recognized any person at her bed-side, answered incoherently the questions proposed, dwelt constantly on the idea of death, for which she fancied she had been so actively preparing, and though her medical attendant was instantly on the spot, it was with much difficulty he was able to apply his measures. Active purging had a beneficial effect, it relieved the urgency of the symptoms. At this time she was attacked with mammary abscess, and had been prohibited nursing the child. Her nights were now sleepless; opium was largely administered to give repose. She became abstracted, and shunned all society; a melancholy, with occasional paroxysms of high excitement, seized her mind; to those whom she dearest loved she showed the strongest dislike, and abandoned herself to despair and to utter ruin, an object unworthy

of the compassion of her Creator. Leeches were now applied, but with no effect upon the mental aberration : this appeared to gain ground ; occasional purgatives were employed, still her gloomy prognostics, her estrangement, and her opposition continued. Change of scene was advised, she was accordingly removed to Cove, and put under my care, at the expiration of five months from the commencement of the illness. Just at this time all her symptoms became greatly aggravated by a severe mental affliction ; she became very irritable, had sleepless nights ; so disliked her children, to whom she was warmly attached, as to forbid their appearing in her presence, and exhibited extravagant gestures. The various functions were imperfectly performed ; digestion impaired, liver torpid, bowels irregular, urine scanty, menstrual secretion scanty and unhealthy, and very offensive—pulse small, feeble, 88 ; tongue stellated, with the prominent papillæ, and rather florid ; face pale but quickly flushed, conjunctiva injected, pupils contracted, expression rather vacant. At the menstrual period there is an increase of many of the symptoms. Under the use of purgatives composed of calomel in combination with compound colocynth pill every second night, and followed, when requisite, with some mixture, she continued to improve for a fortnight, the secretions were amended, and her mind evinced the commencement of a right apprehension of things. However her manner again became excited, and the little improvement observable in the secretions did not continue, the purgatives were still persisted in, with some alteration, and nutritious diet allowed. To this treatment was added the shower bath. At the end of October there was an evident change, the intervals of active excitement became longer, and her manner more rational ; she began to feel a pleasure in occasional conversation, and the cautious introduction of visitors seemed to draw her attention to other objects than the delusions which haunted her. Fearing lest some violence would be done by sanguineous congestion to the brain, which had been for so many months under excitement, I inserted a seton in the nape of

the neck, with the view of keeping up moderate counter-irritation. In the effect I was disappointed, as it rendered her exceedingly obstinate and unmanageable, indeed aggravated the malady; it was accordingly withdrawn. The secretions now so much improved, it occurred, that with a strong hope of success, attention might also be directed to the nervous system; accordingly, while the purgatives were continued, she took every night a pill composed of half a grain of extract of belladonna. The effect of this treatment was highly satisfactory, her nights were visited with refreshing sleep, her skin, which had been parched from the onset of the illness up to this period, became moist, symptom after symptom improved, and, at the end of six weeks, her eye lost its vacuity, her countenance wore a contented and intelligent expression, her mind was calm, collected, and happy, and her feelings so altered, when contrasted, as she expressed it, with her past wretched and dismal state. The belladonna was employed up to the period of her permanent improvement, which was toward the end of December, when, to use the language of her husband, "she was as well as she had ever been during her life." It has been remarked that, generally, this form of mania occurs in individuals whose families have in some branch exhibited a disordered state of mind; perhaps, to the case under notice, the observation may be applied. There is, certainly, no illness or malady which requires and demands the best feelings and exertions of a physician more than the present; the domestic happiness of so many is frequently involved in the result. Other cases may be related, but I have selected this one as having, perhaps, some little interest for the profession from its duration. The first impression made upon the mind was through the uterine system, which, from the succession of processes there going on, works powerfully upon the whole constitution. After a time the hepatic functions became deranged, and in a few days following the period of delivery the particular symptoms were developed. There are several reasons for considering this as a case which betrayed nervous

excitement without power, rather than one of congestion or high inflammation; the distinction in the application of our means is of great importance; the purgative here employed was moderate, as strong evacuants always increased the mental disorder; hence a close watch to the maintenance of the system became necessary. Leeching, when practised, did not in any degree alleviate the mental symptoms. When the menstrual discharge continued there was an aggravation of the malady, and every thing which tended to debilitate assuredly injured. Such was the influence which the nervous disorder exercised over the secretions of the alimentary canal that the suspension for a short time of the use of purgatives was attended with a renewal or aggravation of the unhealthy action to a degree that the disuse alone of the evacuants was not sufficient to account for—as the mind improved this was not so apparent. Opium had here been tried, but, though given in large doses, it had not a favourable effect. The first dose of extract of belladonna taken had a marked influence in calming and subduing the nervous excitement, and to it may be attributed the steady improvement which took place. There are many cases in which belladonna will exercise a more beneficial effect than any other narcotic we can employ, as this present one shows; to be given for a sufficient length of time and with due attention to the bowels, watching its poisonous influence over the system carefully, and it will be found frequently a valuable medicine in our hands. The moral management of puerperal mania or melancholia, is a matter of deep importance, and claims our attention in an equal degree as our application of medicines. From what was originally adopted, there was in this case an alteration which readily showed its suitability to the mental disorder.

OSSIFICATION OF CEREBRAL VEINS.

On the 22nd last September I was called to visit a gentleman æt. 26, who had been a few days complaining. He was a strong, muscular young man, and tried to shake off the trifling illness, as he thought it, under which he laboured. I found him com-

plainings of head-ach, nausea, and general uneasiness, with flushed countenance, loaded tongue, dry and hot skin, and on his arms an erythematic eruption of concentric shape, intermixed with a very few petechial specks. Pulse 112, full and compressible. Bowels were opened by a saline purgative used at his own suggestion. I do not think it necessary to state the treatment which was adopted, and shall accordingly confine myself to the progress of symptoms as they presented at each visit :

23rd. Spent a restless night; head-ach removed, but has had constant ravings, and chiefly on religious subjects. Is flushed and rather incoherent. Tongue cleaner, bowels moved, skin soft, and during night was bathed in profuse perspiration; pulse 132, small, weak; erythematic patches more numerous on arms.

Evening. Passed the day tranquilly, with some inclination to sleep; mind more composed, yet apprehensive; countenance less flushed; tongue moist; pulse 128; skin rather hot and dry.

24th. Bowels often moved, and dejections bilious; night spent with occasional dozes; head hot; tongue with a brownish covering; skin dry; pulse 128, firmer; less agitation of mind; is disposed to sleep.

Evening. Towards the latter part of day an increase of incoherency; some stupor; tongue moist; skin dry; pulse 136, small. As this evening advanced he became exceedingly restless, suspicious, and delirious, and impatient of every kind of restraint; pulse rose, heat of body increased, head became more and more affected, and it required the strongest measures to subdue him. Erythematic patches disappeared.

25th. Had some rest; raved much; does not complain of head-ach, but head is hot; face flushed, eyes staring, pupils dilated; very little conjunctival congestion; tongue quite moist; pulse 144, rather full, yet easily compressed.

Evening. Passed the day with some sleep; is incoherent; other symptoms as this morning; secretions natural.

26th. Bowels moved ; raved very much ; is rather unconscious ; cannot be made to answer coherently ; eyes fixed and staring ; breathing oppressed ; perspires ; pupils dilated ; pulse 150, full ; cannot be got to take his medicine, which he resists with clenched teeth.

Evening. Had two convulsive attacks of a few minutes' duration ; paroxysms of oppressed breathing ; eyes as in morning ; head cooler ; tongue moist ; skin moist ; puffs his cheeks, and laughs sardonically ; pulse 130.

27th. Had no sleep ; constant raving about his professional duties ; eyes more diffused, pupils less dilated ; hearing, which was very acute, now dull ; cheeks flushed ; skin moist ; has little perception ; when spoken loudly to laughs idiotically ; pulse 120, with hardness.

Evening. Spent a restless day, with rolling of the head from side to side, and smacking of the lips ; is quite unconscious ; has resisted every effort to give him medicine ; head hot ; eyes more suffused, and bathed in moisture ; raised his hands frequently to his head and forcibly pressed it ; pulse 140, small, weak ; pupils contracted.

28th. Had several convulsive fits during night, after which he usually made violent attempts to bounce out of bed, accompanied with loud and distracted screaming, and displaying all the gestures and conduct of a violent maniac. This day continued to be spent with little moderation of symptoms, and at 4, P.M. he was seized with a convulsion, after which he became exceedingly restless ; then composed : another convulsion soon followed, which left him powerless and insensible ; in which state, after a few minutes, he expired.

Examined the head twenty-four hours after death. The skull-cap was easily removed from the dura mater ; the usual specks of blood, with a fulness, more than natural, of its vessels, appeared on this membrane. The longitudinal sinus contained only a filament of pale fibrin. On raising the dura mater about two drachms of serous effusion flowed out. The

veins on the surface of both hemispheres were extremely turgid and distended with dark blood, and all the convolutions were filled up, and in many places parted by a jelly-like infiltration, clear, and in many parts speckled with a white opake deposit; pia mater exceedingly vascular. On the arachnoid surface of the dura mater, in a point corresponding to the coronal suture, and within a quarter of an inch of the falx, on the left side, was a deposit of bone half an inch long, and in substance about a quarter; farther back, and on the side of the sinus, was a lamina of bone also. On the right side and central part of the longitudinal sinus, and passing outwards between the dura mater and arachnoid membrane, was a long lamina, on whose surface were borne two cylindrical portions of the same substance, which were applied to, and made a marked impression upon, the opposing convolution of the brain; the one was small, with its canal almost obliterated, the other half an inch long, and a quarter in diameter, still retaining a free channel. Into these bony cylinders the extremities of the two large central cerebral veins were converted. The ossific deposit took place between their coats; by the one the return of venous blood was completely impeded, and ossification had gone on so far as to diminish the diameter of the other vein to one-half its size. In no other part of the veins did I observe an ossific change. The substance of the brain was dotted with red points, and an ounce of pale serous fluid found in the ventricles, the walls of which, as well as the choroïd plexus, were pale.

With the symptoms of ordinary fever this case commenced. He had been ill four days before I visited him. After the first day the head-ach left him, and afterwards, while he retained his senses, he never complained of pain, but of confusion of the head, to which he was in the habit of frequently raising his hands. Through the whole he did not express any particular impatience of light. At a very early part of the illness he opposed the use of medicine. The convulsive paroxysms were short, sometimes general, but frequently one member now, and

then another, would be the subject of spasm. To the last his limbs retained their power ; with the exception of a bad attack of jaundice he always, until this illness, enjoyed good health, and was capable of great bodily exertion. His habits were abstemious, and if he at any time took even one glass of wine it was certainly followed with heat and pain of head, and a degree of excitement sometimes distressing. Manner rather excited and impatient. When a boy he got several falls upon his head, indeed whenever an accident occurred his head was sure to bear all the effects of it. On one occasion, so severe was the fall upon the crown of his head that life was despaired of. Was the ossific action, of which we above see the results, set up in consequence of the severe accidents to which, in childhood and boyhood, he was exposed, and the subject of? That for many years it was going forward there can be little doubt, and had he survived, the canal of the second large cerebral vein would have been, to all appearances, as completely blocked up in a short period as the first. Such a state of the veins, with the mechanical interference arising therefrom, gave to his mind its particular character, and impressed upon him the utility of being in all things abstinent. I have not seen recorded a case similar to this any where ; those mentioned by Doctor Abercrombie, Cases 30 and 133, in his valuable work on the Brain, present not the perfect and distinct ossification here witnessed.

ART. XXI.—*Letter from A. CARMICHAEL, Esq.*

TO THE EDITORS OF THE DUBLIN JOURNAL OF MEDICAL
SCIENCE.

GENTLEMEN,

In your last Number appeared a letter signed "*Anti-Quack*," on the Metaphysical and Phrenological Opinions of Dr. Elliotson, in which my name is very un-

necessarily, and I am led by my friends to think, very inappropriately introduced. If my Essay* had been in the hands of your correspondent, he was at full liberty to assail its author as inveterately as he pleased; but I cannot admit his right to wound me through the sides of Dr. Elliotson. I feel in the highest degree honoured by the notice of that eminent man; and his adoption of any opinions of mine, has given me more pride than will compensate even for the pain of finding that they were not as intelligible to your correspondent, as they were to Doctor Elliotson.

Perhaps they would have been equally intelligible to the former, if he had taken the trouble, like the latter, of reading my Essay. I feel in no wise discomfited, that he gleaned but little information from the three or four lines in which Doctor Elliotson alludes to my opinions; but which, it is obvious, he never intended as a compendium of my argument, but rather as a medium to remind himself or others, of one or two of the leading topics embraced by my Essay. It was from the whole of my work, that *that* celebrated physiologist drew his inferences; and I flatter myself your correspondent would, from the same premises, have arrived at the same conclusions. I would be glad even to bring this observation of mine to the test, and, as it is now out of print, furnish you with a copy for his use. If on its perusal he should see reason to differ from me, let him attack me if he will.—I shall not shrink from the contest, even though my anonymous adversary shall please still to wear his invisible armour. It will be more manly of him, however, to meet me on equal terms. I am not ignorant of the disadvantages which weighed down Sir William Draper, in his encounter with Junius.

It would be impossible, without recapitulating the greater

* An Essay on such physical Considerations, as are connected with Man's ultimate Destination, the essential Constitution of superior Beings, and the presumptive Unity of Nature. Shaw, Dublin, 1830.

part of my Essay, to do justice to the reasoning by which I endeavoured to expose the jejune and preposterous notions so generally entertained of spirit, soul, immateriality, &c. &c., and which have led so many wise men to believe, that a real-substantial existing Being, can glance, like our thoughts, from heaven to earth, from earth to heaven, from the centre of creation to its circumference, and traverse even infinitude itself, in the mere moment in which this can be accomplished by the powers of thinking, without any relation to space or to time. Thought, and every modification of thought, whether expressing propositions *true* or *false*, is, no doubt, *immaterial*. Still it is merely an *operation*, and not an *entity*; yet your correspondent, accomplished metaphysician and able logician, as I freely acknowledge him to be, falls into one of those vulgar errors which it was the business of my Essay to denounce, that of confounding mental conceptions and abstractions with real entities. He observes that TRUTH is perfectly distinct and abstract from matter, and once produced, is everlasting and immortal; and he calls it in express terms, “an *entity* which can never cease to be;” adding “it has a metaphysical, though not a corporeal existence, and wants only a *consciousness* of individual existence, to make it, to all intents, what we mean by AN IMMATERIAL SPIRIT. And (he asks) who will say that it is beyond the power of Omnipotence to give to this IMMATERIAL ENTITY, or any combination of such, this *consciousness* of existence which would perfect its personality and identity.” (p. 177.)

Adopting his language, without assenting to its propriety, permit me to observe, that this “immaterial entity,” TRUTH, is, in its essence, thought; consciousness is also thought; to give Truth consciousness, would be to make THOUGHT think. To make the soul, if soul there were, think, or to make the brain think, is a tenable proposition; and we have the strongest reason to believe, that the latter is what God has already done; but to make Thought think, or Motion move, would be rather

an inconceivable task to impose upon Omnipotence. I acknowledge, therefore, that it is to me inexplicable, how Truth, however immaterial and immortal, is to acquire the personality and identity of an absolute entity. But (to turn to account the words of your astute correspondent) though not very intelligible to me, it may be very conclusive to him.

In my attempt, however, to ascertain the extent of belief which we may rationally entertain in the present state of human knowledge, on subjects so interesting and momentous to the species, I arrived at conclusions very different indeed from those which still continue most prevalent; but which, since the publication of my novelties, are not (if I can so express myself without the imputation of arrogance) so universally or pertinaciously cherished as they were. The most important, and, at their first promulgation, the most startling of these conclusions was, that GOD IS THE ONLY SPIRIT IN EXISTENCE; the only Being who perceives, and thinks, and feels, and wills, and enjoys, throughout the entire of his substance; in reference to him we may call it his *infinite substance*; or as more elegantly expressed by Newton, “*Deum summum necessario existere in confesso est. Et eadem necessitate semper est et ubique. Unde etiam totus est sui similis, totus oculus, totus auris, totus cerebrum, totus brachium, totus vis sentiendi, intelligendi, et agendi; sed more minime humano, more minimè corporeo, more nobis prorsus incognito.*”*

The next in importance was, that all other Beings, however exalted their nature and powers, are but organized beings, adapted to accomplish the ends and purposes for which HE designed and framed the exquisite machinery of their organization; and that this great universe, consisting as it does (notwithstanding the solecism) of thousands of universes, is in its “many mansions” also substantial and material, as fitted to the support of its substantial and material inhabitants; and in every respect

* Principia, lib. iii. Scholium Generale.

unlike that "spiritual world," as we are pleased to designate it, which in our overweening bounty we have added to the solid, stupendous, magnificent creation of God ; presuming that he had not done enough for his own exaltation, but must be indebted to our pitiful help for such augmentation of his Omnipotence as will satisfy our sublime and vast understandings.

Co-relative with the foregoing conclusions is another of equal interest and importance to us, that man does not possess a double nature, or a double passport to eternal life ; and that he must depend, not on an immortal soul, but on the will and the power of God to raise him again from death to life ; that the two hypotheses are distinctly and irreconcilably incompatible ; wherefore, if there be an immortal soul, there is no resurrection ; and if a resurrection, there is no immortal soul. If, however, we have still a doubt which to choose, let the Scriptures decide our choice ; and we there find the resurrection of the dead the very essence of the Gospel—thus affording a satisfactory proof of the divine origin of this boon to man, when truths, the most recondite and difficult to believe, are at once confirmed by the inductions of philosophy and the dicta of Revelation.

I confess that without the general views to which I was guided by that Phrenology, which is so abhorrent to your correspondent, I could never have found my way through the labyrinths of metaphysics to these speculations so simple and magnificent, that now, to my eyes, although attained by strict induction, are almost self-evident. It is true I have made no allusion in my Essay to this science. I knew that the truths I advocated were unpalatable, and I was unwilling that they should bring any odium upon truths equally unpalatable, but far more important ; I repeat, more important, because advantageous in their practical uses, while mine are only advantageous in contributing to the acquirement of just notions, in place of absurd ones, on speculative but most exalted subjects. But there is no reason why either should be unpalatable ; a future state is as certain to him who believes in

the resurrection from the dead as to him who confides in an immortal soul ; and equally assured to him who believes that the various powers of the mind are manifested by different portions of the brain, as to him who believes that *that* brain is a useless appendage bestowed by the Deity for no purpose whatever ; and that Man could exercise every mental power he possesses as vigorously and effectually, were his skull but an empty cavern, in place of being occupied as it so wonderfully is. But it is necessary to observe, and with some emphasis, that my opinions may be entertained by those who reject Phrenology, and rejected by those who most strongly adhere to that science. They are altogether distinct and independent of each other.

I enter into no argument on either of these subjects : I must refer to my Essay for the reasonings by which my opinions are supported, without a knowledge of which, any controversy arising out of the mere conclusions here enumerated, would be but labour in vain—again raising ghosts of objections, already laid at rest ; and in the same manner I must refer to the writings of Phrenologists for an answer to the phrenological objections of your intelligent, but, on this occasion, very incompetent correspondent. All that is even plausible in these objections has been over and over again advanced, and over and over again refuted. Twenty years ago they would have had the grace of novelty, and might, for a day, have had their weight ; but it is obvious that the present disputant knows but little of the science he assails ; and *that* little he has acquired to little purpose, not for the sake of knowledge but of disputation ; he disregards the facts of Phrenology, and turns aside from the reasoning which has established it as the true science of mind. But why quarrel with a science which consists more of facts than of inferences ? (see p. 197.) Is it not on this very account the more certain, if its facts be facts ? Surely it is as much a science of observation as astronomy, or chemistry ; and I would as soon think it necessary, in its present state of maturity, to defend the recorded and uncontradicted facts of the *one* as of the *others*. If the uninitiated

in chemistry and astronomy cavil at the fundamentals of these sciences, let them acquire a little more information and they will cease to cavil. Let the uninstructed in phrenology go and do likewise, and the world and themselves may be benefited by a similar result.

I cannot conclude without observing, with a strong feeling of mingled surprise and regret, that so able a writer as your correspondent, should attempt to throw on Doctor Elliotson, throughout the whole of his strictures on his metaphysical opinions, occupying from page 170 to page 179, without the slightest qualification, the uncandid imputation of attempting “to subvert the solid foundations on which public and private happiness most securely rests, namely, the existence of a *future state* of reward and punishment.” I cannot demonstrate the injustice of this charge more effectually than by transcribing an ungarbled passage from the very page of Doctor Elliotson's invaluable work on Physiology, (360,) to which your correspondent refers in the heat of his argument on this, as it seems, exasperating subject :

“The mind is evidently the property of the brain, and the operations of the mind, whether relating to sensation, will, intellect, or affections, are evidently the operation of the brain. In the division of this work devoted to general physiology, I proved the brain to be the organ of the mind, as much as the liver is the organ of the secretion of bile ; that what holds good of the function of every other part, holds good of the function of the brain ; and that to ascribe the power of the brain to an immaterial imaginary something called a soul, is a mere hypothesis, the remains of unenlightened times, and not only unnecessary to THE BELIEF OF A FUTURE STATE, THROUGH A DIVINE REVELATION, but calculated to throw discredit on such revelation, by making its annunciation of a future state appear superfluous.”

To these views I give my unqualified assent ; and only wonder how his opponent, graced with science as he is, could so abuse the privileges of controversy.

There is still one other observation of your correspondent which I cannot neglect, in which he argues that Dr. Elliotson has entered into this dispute most gratuitously ; and more than argues, asserts that “it has no imaginable bearing upon any one of the questions, or uses to which his professional labours tend, or about which they are properly conversant.”—(p. 171.)

Surely Doctor Elliotson was, upon this very occasion, treating as a Physiologist, of the mental functions of the nervous system ; and was actually engaged in pointing out the special operations of the brain. If these special operations were the various modifications of thinking and feeling, desiring and willing, and that these were, in his opinion, produced by the brain alone, without the intervention of any auxiliary Entity, material or immaterial, corporeal or spiritual, through the mere force of the powers given by God to this marvellous piece of machinery, was it not his duty to teach the world according to his knowledge and his convictions ? Or would it have better pleased your correspondent, who avers, that he himself is “as much as man can be, for freedom of discussion, and for truth, whatever may be the result,”—would he, I repeat, have been better pleased to see this professed and accredited teacher of the phenomena of nature, skulk like a coward from the announcement of a truth of which he was persuaded, not only in his understanding, but his conscience ? Or because your correspondent conceives, “that it is *not certain* that the popular opinion is false, but that *it is* certain that *it cannot be established to be so* by those who arrange themselves against it,” (171,) is the human mind never to make an effort to shake off these popular opinions, although convinced of their falsehood, or to grasp at the Truth as it temptingly manifests itself, although for aught your correspondent supposes to the contrary, it may be fairly within the reach of the human intellect ? are we for ever to give up these sublime speculations, most interesting, most momentous to man, because there are some hypocrites that

would not, and some dastards that dare not attempt the prize, or even countenance the attempt? Give me THE LIGHT OF TRUTH whatever be the consequence—

——— δὸς δ' ὀφθαλμοῖσιν ἰδέσθαι·

Ἐν δὲ φάει καὶ ὄλεσσον, ἐπεὶ νύ τοι εὖαδεν οὕτως!*

But does this writer imagine, that in the advances of human knowledge, we can possibly light upon a Truth pernicious to man, or degrading to God? away then with these slavish and profligate barriers, that would shut us out from the vision of God's works AS THEY ARE, and thus impiously debar us from the only means within our limited sphere, of attaining a just knowledge of that wisdom, that power, and that goodness, which constitute His eternal and infinite glory.

I am &c. &c.

ANDREW CARMICHAEL.

* Iliad. xvii. 646.

BIBLIOGRAPHIC NOTICES.

Plague, from actual Observation. By DOCTOR BULARD, (de Méru.)

DOCTOR BULARD, as well as many others of his countrymen, sought Egypt at the breaking out of the plague in 1834. There he found already Aubert, Boyer, Lachèse, Gætani, Clot, Duvigneau, Perron, Pacthad, Dussap, Rigaud, Reymont, and Fourcade: the four latter of these physicians fell victims to the disease which they came to combat. It was the intention of those gentlemen, by proper regulation and ordering of the hospitals, by the employment of such remedies as science affords, and by accurate observation, to struggle with this enemy of the human race, of which a just idea cannot be formed except by those who have seen it.

It is well known in Europe, how little Mehemet Ali prized this spirited undertaking which devoted so many young men to a land, the inhabitants of which were allied to them alone by bearing the forms of men. One day Doctors Bulard, Clot, Lachèse, and Gætani were admitted to an audience of Ibrahim Pasha, and when he was informed how Doctor Bulard had exposed himself in every way to contagion, except lying with those affected, that savage warrior only remarked, "well, what does all that prove, but that he is the greatest fool of the four!" When the disease had subsided, these gentlemen were neglected in the most shameful manner, because their services were no longer necessary. Doctor Clot alone, who had not distinguished himself more than the others, was overloaded with honorary distinctions, and elevated to the rank of a Bey. Doctor Bulard was ordered to march with the troops to Hedshas, where he could only expect to be massacred by the Bedouins, or to die of the boils of Yemen; liking neither of these alternatives he demanded and received his congée. This young physician next proceeded to Smyrna, where plague was raging, and proved his courage by daily communion with, and treating of an immense number of sick, by making *post mortem* ex-

aminations of 300 bodies, till at last he became convinced of his personal immunity from the disease, after wearing for sixty hours a shirt taken warm and speckled with blood and matter from a patient.

At Smyrna he found the hospitals in the most shocking condition. The sick without attendance and wanting every thing; the dead and living mingled together; a wretched supply of food; and thus the salutary influences of the curative powers of nature were completely cut off from them. The surgeons cut out the buboes of the sick, but at the same time severed the arteries, so that those whom the disease spared were sure to die of hæmorrhage. Thus the hospital was only the ante-chamber to the grave, and its accounts were easily and accurately kept, for up to the 16th of May, 210 sick had entered it, and 210 had died.

Such was the condition of things when Doctor Bulard's arrival was hailed with joy by the principal inhabitants, and he was given charge of the Greek Hospital. The impression made by his appearance on the miserable patients is not to be described, they wept, prayed, and endeavoured to press their burning lips on the hand of the physician, &c.

Doctor B. lost no time in regulating the confusion, and by the following morning this was accomplished. The invalids were separated from those who were severely affected, and placed in a ward by themselves. The house was cleanly scoured, aired, &c., and a dietary established, bandages applied, and medicines administered, attendance night and day provided, and the whole service of the hospital put in order.

From the 15th of May to the 27th, the number of patients in this hospital had quadrupled. During this period eighty cases were treated; twenty died, the greater part of which had been in hospital previous to Doctor Bulard's arrival; seven were discharged cured; thirty-five were in the invalid hospital; seven remained dangerously ill; the rest were recovering. Doctor B. distinctly guards against attributing these favourable results to any specific plan of treatment, but thinks it owing to the moral influence on the patients of extreme cleanliness, a better diet, attendance, and perhaps some medicines.

From that time the general state of the hospitals at Smyrna was much improved.

In the middle of June, towards the cessation of plague, Doctor Bulard made a new experiment of inoculation. Experience had taught him, that the period denominated by him critical, and a favourable change, leading to a probability of recovery were synchronous, when at the commencement of the

disease, carbuncles with broad bases made their appearance, and when the buboes in the groins and axillæ were developed quickly, and ran on speedily to suppuration. Grounding his experiment on this observation, he inoculated those patients in whom these symptoms were wanting, causing thus artificial buboes and carbuncles, and the majority of persons so treated recovered. In order to discover what was the positive mode of action; what general or local irritation matter from these artificially produced buboes or carbuncles would produce on a healthy person, Doctor B. determined to collect the matter of boils produced by inoculation, and to inoculate others with it. By this he hoped to discover, first, whether the inoculation of matter from an artificial bubo or carbuncle would produce in a healthy person symptoms of absorption; and if so, secondly, to determine whether the bubo or carbuncle thus produced were similar to those of plague; and third, to learn whether the inoculation of pus or serosity from the true plague bubo, always produced similar effects or differing in intensity.

To satisfy these doubts, Doctor B. introduced into the cellular tissue of his own groins, the slough from a plague bubo, and thus produced a carbuncle. For twelve hours he suffered a violent and constant pain, until the slough was perfectly formed. Then there was violent general excitement; fever, with pulse from ninety to ninety-five; next day the pulse returned to eighty, and remained thus for two days. The lymphatics now swelled; copious perspiration came on; and general symptoms which quickly disappeared with the perfect development of the local affection. At the lower part of the opposite groin, matter from a bubo was introduced by a similar incision, and the wound healed by the first intention. To this succeeded only a slight and imperfect process of inflammation, perhaps, because the incision although an inch deep, was not deep enough.

Whatever else may be thought of these experiments, they must be considered very brilliant evidences of daring and courage. The manner of this inoculation may be objected to by those who consider the method in which the variolous and vaccine diseases are best communicated by merely slightly scratching the skin, but the object here is very different, not that of superseding one disease by a more powerful influence, but locating a general disease.

Doctor B. must have left Smyrna about the middle of July, and we should rejoice at his finding, on his return to Europe, the already widely spread reports of his heroic daring, nor

would we be the last to hail his boldness, and rejoice at the immunity he has been permitted to enjoy in his scientific but perilous research. We will now subjoin in a condensed form the observations he has recorded on this destructive disease.

In arguing on the endemic and epidemic occurrence of plague, it must be understood that our conclusions are drawn only from a retrospect of the localities which we have personally observed, and that we have no wish to give any opinion on those as yet unexplored by us. But we will say that we are disposed to believe in an existing endemo-epidemic predisposition in those places where plague usually appears; as yet we do not thoroughly know those localities and their medical constitution. The facts hitherto communicated, speak positively for contagion; let us now compare them with the authentic records of our own private observations. In the riding-school at Ghizeh which contained 600 persons, and where the sanitary regulations were maintained with military precision, during six months not a single case of plague occurred, although the disease was raging and committing devastation at the foot of the walls, so that in a population of from 8000 to 10,000, sixty or eighty died daily.

The Palace of Schubra, where Mehemet Ali, with a suite of 300 persons, dwelt, was surrounded with a double sanitary cordon, and a line of military posts. Within this circle no case occurred, either during the period whilst the malady was increasing, or for three-fourths of the time whilst it was decreasing; whilst in the village of Schubra numerous cases occurred, both amongst the inhabitants, and troops stationed there, and it was only when the disease was nearly extinguished that three cases appeared in the palace, which circumstance those theorists who esteem plague epidemic would certainly lay hold of, but for the declaration of Doctor Cartagnoni that the quarantine was broken, first by some eunuchs who by command of Säid Bey had communicated by a private door in the back yard with those without, and next by the gate-porter of Osman Bey, the Nazir of the garden at Schubra, who during the night climbed up a scaffolding, gained over the watch, and proceeded to a house where his daughter had just died of plague, and having taken all the valuables which he could find there, returned into the quarantine before day break. Immediately subsequent to this two eunuchs died, having gone through every stage of the disease.

In the artillery school of Tara at Ter-el-neby in Casserlein, in the Polytechnic School of Buloe, in all those large institutions where great numbers of persons collected together must be much exposed to the influence of an epidemic, yet where

the strict observation of salutary precaution was observed, the disease never occurred.

The Harem of Scherif-Pascha which contained more than sixty persons, perfectly escaped by the observance of strict quarantine. Whilst, on the contrary, in the next house, or rather in part of the same house which was allotted to the Mamelukes, and where there was free communication, out of twenty Mamelukes nineteen died of plague.

Finally, ninety-nine in 100 of those places in which quarantine is strictly observed, perfectly escape this terrible scourge.

At Smyrna this truth was doubly proved, there was immunity wherever there was perfect separation, and devastation wherever there was free communication; yet every effort is made by those who possess influence there to draw exactly opposite conclusions. Whether it arise from fatalism, indifference or disinclination, the Turkish population never adopt any precautionary measures, and the comparative mortality is excessive in it, when compared with that of the other parts of the city amongst the Greeks, Catholics, Jews and Armenians. The following table will sufficiently indicate this fact.

Table of the Effects of Plague on the Population of Smyrna for five Months.

ABSOLUTE NUMBER.				COMPARATIVE NUMBER.		
In a population of	Cases.	Died.	Cured.	Cases : Pop.	Mort. : Cases.	Mort. : Pop.
Turks	58000	4500	4000	500	$\frac{1}{13}$: $\frac{8}{9}$	$\frac{1}{14\frac{1}{5}}$
Greeks	48000	600	450	150	$\frac{1}{80}$: $\frac{3}{4}$	$\frac{1}{106}$
Catholics	10000	50	30	20	$\frac{1}{200}$: $\frac{3}{5}$	$\frac{1}{333}$
Jews	8000	457	297	160	$\frac{1}{18}$: $\frac{2}{3}$	$\frac{1}{27}$
Armenians	6000	120	54	77	$\frac{1}{50}$: $\frac{3}{7}$	$\frac{1}{111}$
Total	130000	5727	4831	907	$\frac{1}{22\frac{2}{3}}$: $\frac{4}{5}$	$\frac{1}{26\frac{1}{2}}$

Now, should any one suppose that this difference does not depend on the absence of sanitary precautions, but on the manner in which the Turks live, such supposition would not be tenable. The upper part of the city, almost entirely inhabited by them, lies on the northern aspect of a hill in a healthy situation, protected from the south. The place is paved, the streets well ventilated, the houses roomy, and the inhabitants, by the rules of their religion, are cleaner than most of the other persuasions. In what circumstance, then, are we to seek, with such physical relations, for the cause of the prevalence of plague amongst them? The lower city, on the contrary, which

is principally inhabited by Franks, is a perfect dunghill, and possesses all the disadvantages possible for a healthy residence. "It is," says Dr. Floquin, in his *Considerations on Typhus Icteroides*, or the yellow fever of Smyrna, "cursed from its low situation to be the receptacle of all the sewers which empty themselves along the quay of the Rheda, which passes the most respectable part of this quarter of the city. At the north-east, the point which is most distant from the houses of the upper city, the lower quarter is surrounded, during a part of every year, by a morass; this, during the rainy season, is constantly covered with water, but when the great heats come on, it dries and rises in exhalation." Notwithstanding all this, the mortality, taken for five months in this quarter, amounted, when compared with that amongst the Turks, to only one-sixth. Numerical calculations are the only methods which can be had recourse to, to prove the real reasons for these great differences; the cause, as we esteem it, is purely individual; in fact in the upper city there is free intercourse, whilst in the lower more caution is observed.

To plunge a little deeper into the consideration of the destructive effects of plague, we have traced the actual working of contagion thus. We have observed that consecutively for many days bodies were brought from the same house, from the same chamber; it was the master and his servant, the wife and husband, the sister and brother. It rarely happened that a case occurred in any house but it was followed by a second, and a third. In short, when we consider these necrological evidences, we will soon cease to speak of pestiferous localities and a deadly atmosphere. Whatever may be the effects of locality, the representations are so different and so contradictory, particularly with regard to Egypt and Smyrna, that I would deem it to possess but slight influence on the malignity or mildness of the disease. Is it possible, in this manifest advance of the disease from person to person, to defend the opinion that the plague is only epidemic, and carried in the atmosphere? We do not believe it. Some, in order to set aside the undeniable facts of contagion, assert that numerous cases of sporadic plague have occurred, and they declare that it is endemic in Egypt. But, independent of those cases wanting confirmation, are they even sporadic? They attack only certain individuals, they are unconnected with an epidemic influence, and their effects are confined to a few mild buboes, of which the patients never die. In order also to prove its epidemic nature, these observers have adduced cases of its occurrence in harems and certain private dwellings, which have been considered as kept under strict quarantine. We do not doubt the occurrence of these cases, but it is strange

that they are never recorded to have occurred before the second month from the breaking out of the plague, and we can readily believe that there was some breach of the quarantine which gave rise to those cases, for if the disease was epidemic such places would be affected as well as others at the first outbreak of the disease. The Arabs, like the Turks, are fatalists, and consequently reject the doctrine of contagion, and neglect all sanitary precautions, of which, from their belief, they can see no use, and amongst them the devastation is very great. Examples of such neglect are abundant in Egypt, and well known. These facts also are as well explained by the doctrine of contagion, as by the supposition of an epidemic, which is not supported on any logical basis, or strong argument; and the favourers of the epidemic doctrine, as if to invalidate their own reasoning, adduce the lightness of the attacks in a number of those cases, just as if the effects of contagion were not as relative as the epidemic influence, infection, or any other inoculation. In a word, the impartial examination of facts will not admit of belief in the endemic or epidemic origin of plague in Egypt, or Smyrna, because they absolutely prove its contagious nature.

Now one word for the miasmatists. In order to establish their epidemic theory, they had to discover an exciting cause; this naturally presented itself in the overflowings of the Nile, which, from the long stagnation of its waters beyond the bed of the river, leaves behind it, when it retires, a deposit of slimy mud. From this deposit (say they) arise exhalations carrying the originating principle of plague, which, at an undetermined time, exercises its effects on the respiratory organs of those in whom the poisoning exhibits itself. If this were conclusive, the plague would also be epidemic in Lower Egypt, and there also the overflowings of the Nile might be recognized as the cause, the exhalations from its deposit as the primary principle, and infection as the means by which it should become endemo-epidemic.

In contradiction to those opinions, we say, that the overflowing of the Nile takes place yearly, and yet the plague was absent two years from Egypt, in 1829 and 1836, and yet in those years the inundation was extraordinarily great. If the poisonous miasma from the deposit were really the cause, these were the periods, of all others, when it should have acted with greatest virulence. The numerous villages which form, as it were, so many islands in this deadly morass, should have been the first places affected and the ever open theatres of the disease, from the mephitic atmosphere with which they were surrounded. The Fellahs who live upon the deposit left by the river, and whose cattle break loose and stray about at the first commencement of the

retreat of the inundation, and whose whole existence depends on the fruitful qualities of this bed of mud, do not find death in pursuit of their livelihood, and it has never been remarked that they are the first victims of plague. With regard to numbers, the mortality is not found to be greater amongst this pastoral people from plague, than it is in the cities, but on the contrary much less, and frequently unimportant. According to the latest observations the plague, when not actually existing in Egypt, always makes its first appearance in a maritime city. The first cases of plague always occur in Egypt with the arrival of the pilgrims on the coast, and their journey through the land, to arrive at Mecca. Since the memory of man, the plague has been always introduced into Egypt from Constantinople, or from different parts of Asia. Since sanitary precautions were observed along parts of Egypt bordering on the Mediterranean, the plague has retreated before them, and has been confined to those parts by which the caravans travel from Constantinople, Syria, or the islands of this horn of the Mediterranean.

The plague of 1834 succeeded the introduction of goods into the Greek convent, which were favourable to the conveyance of contagion. The plague in Egypt is only commencing whilst it is raging on the shores of the Bosphorus. The plague always extends from the circumference towards the centre of the circle, from the sea towards the inner part of the land; the caravans pursue the same course from Alexandria, Rosetta, Damietta, and the internal parts of Syria, from place to place, as far as Cairo. Thus plague extends over all parts of the Delta, and stops short at the farthest bounds of Upper Egypt. Not one single well authenticated case of plague, arising from a purely epidemic cause in any closely observed private quarantine, or at the commencement of the disease, can be offered or proved. We have made experiments with the clothes of persons affected with plague, and deduced positive results. Whilst those places where strict quarantine was observed have remained secure; those where free communication existed always gave evidence of contagion. These sixteen conclusions are, in few words, what Doctor B. draws from a six months' dispassionate examination of this disease, which he, with justice, states has hitherto been involved in a maze of hypothesis.

Symptomatology.—The specific character of plague is sufficiently shewn by the manner of its appearance and its extending. Yet, when we come to compare it with a chain of other relations of a similar kind in other diseases, we will then see to what point symptoms may be mistaken for diseases, and *vice versa*, whether a particular locality is essential, or whether, in opposition to the great fundamental principles of the latest solid pathology, a new

pathology be not necessary, from the want of agreement between disease and symptoms. In fact, there remains to be discovered some better pathology than irritation, some other reason than locality, and some other treatment than the antiphlogistic.

Plague has no premonitory symptoms. Its course may be divided into three distinct stages; first, the period of attack or seizure; second, the reaction or inflammatory stage; and third, the critical period. At the commencement, those affected commonly feel of a sudden an extraordinary uneasiness in the nervous system, with sensations of general weariness, shuddering, headach, more or less violent, a dull throbbing, and on motion, considerably smarting pain in the axilla and groins, sometimes nausea, followed by vomiting. The countenance exhibits an expression of feebleness, the eye is downcast, the eye-lids half closed, the mouth half open, and the gait is staggering and uncertain, like that of a drunken man. Some hours later the depression is excessive, the limbs are relaxed as if dislocated, standing or walking is now impossible, the head remains sunk upon the breast, the skin is hot and dry, the pulse frequent, and miserably small, beating from 115 to 130, the breathing is tumultuous, and as frequent as 30 to 35 times in the minute; the tone of the voice is not changed, but articulation is impeded and rendered difficult, as in a person oppressed with fat, so much so that one might suppose the sufferer afflicted with angina, which never is the case. Sometimes the patient is perfectly speechless; the tongue is always moist, broad, white in the middle, and shining almost like mother of pearl, cleaner at the point and on the sides. Vomiting occurs, apparently without the patient being conscious of it; the ejected matter is often bilious, seldom green, and sometimes consisting merely of the fluids which have been swallowed. At the period of greatest intensity of this stage, the sick fall into a kind of comatose condition, the skin is dry and rough, with less heat, and the pulse is very small, frequent, or not perceptible; the breathing becomes slow and irregular; vomiting occurs seldom, the jaws are pressed firmly together, the lips are livid, the limbs lose their warmth, and the patients die.

In a great number of cases these stages did not exhibit those violent and dangerous characters which we have described; there are no positively general symptoms. In lieu of all the other pathological appearances buboes are seen in the usual places, and sometimes a moist, flattened, and pearl-coloured tongue, with a slightly feverish condition. The general organism remains unaltered, the buboes suppurate or dissipate, and thus the disease terminates: this has been called the benign plague.

But when the disease is developed in its full strength, then almost all those affected perish from the second to the fourth day.

In some cases it happens that the number and intensity of the symptoms are diminished, or some are produced prematurely; the nervous symptoms disappear, the vomiting ceases, there is an increase of respiration and of pulse, and abundant perspiration comes on; from the general illness there now remain weakness, and a slight state of irritation of the digestive organs; the lymphatic affection, however, maintains its ground, and proceeds to induration, or suppuration, or subsides, and the patients recover. This is, however, rare, and may be computed at about $\frac{1}{25}$ th or $\frac{1}{30}$ th of the persons cured.

In all cases the following diagnostic symptoms occur either singly or collectively:

1st. Knotty tumours of the lymphatics in the groins and axillæ, rarely in the neck, and still more rarely about the knee.

2nd. Petechiæ on the thorax, the neck, and sometimes over the whole surface of the body, rarely on the limbs.

3rd. Carbuncles in greatest number upon the limbs, but seldom upon their extremities, or upon the face, or trunk of the body.

At Cairo, and particularly at Smyrna, these three morbid appearances have constantly indicated three different forms of disease: the simple, where buboes alone occur, that in which buboes and petechiæ occur together, and that in which buboes and carbuncles are united, but we have never seen those two last forms occurring together.

After the first period reaction almost always follows, then a new series of symptoms appears, and the disease changes its aspect completely, the empty state of the arteries disappears, the pulse again returns, rises, is hard, full, and about 90 or 100 in the minute; the countenance becomes lively, excited, sometimes indicates strong expression, the eye-ball again acquires the power of motion, which, in a great degree, it had lost, the conjunctiva becomes clear, the pupil enlarges, the tongue becomes dry, then brown, cleft, and like horn, the teeth are sooty, those parts of the lips where mucous membrane exists are covered with crusts, the nasal apertures are filled with a black, tenacious stuff, which turns into dust as it dries. When any thing is ejected from the stomach it is always blackish and tolerably thick; the skin retains all its dryness and hardness; the breathing is frequent, the voice elevated; general agitation; often disturbed dreams, and wandering of mind, but seldom real delirium. Obstinate constipation is usual, but in some cases, which may be considered as exceptions, there is a very

offensive black discharge from the bowels, without any pain in the abdomen. If the case be about to prove fatal, the excitement goes off, the pulse sinks, and becomes quick, small, and irregular, the breathing laborious and irregular; sometimes there is spasmodic hiccough, the skin is contracted as it were spasmodically, cold perspiration breaks out on the face, and death comes on without a struggle. The medium duration of this period is about four or five days, it is sometimes prolonged to twelve or fourteen days, but in such cases it assumes the typhoid type, with continued fever, subsultus tendinum, some slight spasms, immobility, stupidity, &c.

Crisis.—This period generally occurs in the commencement of the second stage. It is announced at first by a general relaxation which follows the state of excitement; suddenly carbuncles appear with broad surfaces, sometimes four or five inches in breadth, particularly in those regions where chains of glands exist. The buboes enlarge, become more active in their course, and hasten to suppuration, the critical pulse is from seventy to eighty, and preserves a great degree of equality even to the termination of the disease. The skin loses its dryness and harshness, and is in a few hours bathed with a plentiful and continuous perspiration, and sometimes there bursts out a general eruption of a papular or vesicular character: circumscribed red blotches, suppurating swellings, bleeding from the nose and vagina, and premature labour, form the other critical symptoms of this period of the disease. The sooty coat of the tongue cracks, becomes moist, and falls off: the injection of the sclerotic and the dilatation of the pupils disappear, the pulse again becomes natural, the other symptoms yield, and recovery ensues.

What conclusion is to be drawn from such numerous, involved, and dangerous symptoms, which arise so suddenly and which so often terminate fatally? With the results of treatment and the deductions from pathological anatomy, we can only answer that the relative value of any amongst them is very trifling, but that their general consideration makes them of some importance as characteristic of a complicated affection; for, whilst they exhibit the engagement simultaneously of many systems, still they are not pathognomonic of any organic affection in particular, as are the symptoms in pleuritis, hepatitis, &c. They are equally useless in estimating the disease, its nature, situation, danger, or consequences, and still more so in leading to any method of cure.

Prognosis.—The prognosis is to be made according to the absence or presence of some symptoms, and to the stage of the disease. Thus there is always some hope when perfect reaction oc-

curs, merely because the disease is protracted, and a crisis may occur.

If there be no vomiting, or if it be trifling, if the respiration does not correspond with the smallness of the pulse, if there be no cerebral disturbance, nor an eruption of petechiæ, if carbuncles appear in great numbers, and with broad surfaces, if the buboes suppurate quickly, by so much will the prognosis be the more favourable. The spontaneous appearance of symptoms, produced by the united action of the organs, is almost always favourable, but unfortunately very rare.

Plague appeared to us in its most frightful forms, in strong, sound constitutions, in childhood and youth (without sex producing any marked difference.) Whilst, on the contrary, it appeared to us to spare those whose constitutions were debilitated by early dissipation, the aged, the weak, and the sickly.

It is plain from what has preceded, that the symptoms give no information with regard to the termination, derivable from the seat and nature of the suffering; pathological anatomy can alone clear up this weighty question, and we will therefore now carefully give the results which 300 dissections have exhibited to us.

Pathology.—The doctrine of predestination, pusillanimity, or ignorance, which for centuries have deprived its observers of the means of drawing a true picture of the signs and symptoms of plague, have also reserved it for the present time to give an accurate description of its anatomico-pathological history. I have been more fortunate than my predecessors in being able to procure these examinations, if not without danger still without difficulty.

In order better to understand what has been left to us from the treasures of former times with respect to this subject, we will quote from Joseph Frank the review of the post mortem examinations which have been made since 1656, when they were first instituted with some degree of attention by the authorities at Naples.

“The intestines were found gangrenous, and the heart, lungs, and liver were covered with black specks. The gall bladder was full of dark, thick, tenacious bile; the blood-vessels clogged with dark fluid blood.”

Amongst later writers Pugnet has particularly distinguished himself; the following is the result of his observations:

“The brain was so completely softened that it was impossible to examine any part of it; the volume of the heart, particularly of the right ventricle was increased, its walls were soft, and white like the pericardium. The circumference of the stomach was increased, its inner surface covered with mortified

spots, the same appearances occurred on the inner surface of the ileum: the liver and spleen appeared larger than natural; the gall bladder distended with bile of dark yellow colour. The lymphatics could be easily seen. The arteries were empty, the veins clogged with blood; examination has not yet shewn whether the buboes have their seat in the glands, or in the cellular tissue surrounding them."

From this point our examinations begin. The bodies were examined half, one, two, three, four, five, ten, fifteen and twenty hours after death.

External Organs of Sensation.—In white people livid specks of large size, and in greater or smaller number, were almost constantly remarked on the anterior part of the neck and upper part of the breast; the scrotum and labiæ presented the same appearance. We may infer that this also occurs in people of colour, as well in the brown skin of the Arab, as the black of the Æthiopian and the copper-coloured of the Abyssinian. When these petechiæ remained during life, there was always a sinking in of the buboes and carbuncles, a sunken countenance, neither swollen nor livid, eyelids perfectly closed, nose and mouth generally covered with the dark matter which has been vomited.

Muscular System. Very slight cadaveric stiffness; muscular cohesion much diminished; the whole muscular tissue, soft and somewhat livid, and a little discoloured.

Nervous System.—Of all parts of the organism, the nervous system is the one which is found most normal. The sinuses of the dura mater and all its vessels are distended with black blood. The membranes are free from disease. The white substance, when sliced, exhibits a vast number of black bloody points; the grey substance is pale. The cerebellum exhibits the same characters. The consistence of the whole mass of brain is generally decreased. The ventricles and choroid plexus shew no degeneration, and contain very little serum. The sympathetic nerve is neither red nor softened, its ganglia are always healthy, and it is only in some rare cases that petechiæ, or rather an exudation of blood, have been seen on it in the lower part of the chest, but when closely examined, these specks are found to be only of the thickness of the neurilemma, and do not pervade the tissue of the nerve.

A second remarkable change which the neurilemma undergoes, is in size, where the nerves are included in knotty swellings of the lymphatics. When these swellings are much developed, and when there is much blood in the part, then the outer surface of the neurilemma is observed to be much covered with those specks; but if a slight incision be made, and if it be care-

fully dissected, then it may easily be seen that these specks are confined to the external layer of the neurilemma, and to the cellular tissue surrounding it, and that consequently they are only proofs of a kind of imbibition.

The different plexuses of nerves, and particularly the solar, are without any appreciable alteration.

Lymphatic System.—The only constant morbid alteration, which at the same time is the most general, the deepest, and the least known, is, without doubt, that of the lymphatic system. The glands exhibit disease either by the excess of their development, by their high colour, or by various other pathological changes. If all the autopsies which have been made, be considered at once, these glands will be found to have varied in size, from that of a small pistachia nut, to a goose egg, and even larger, and in colour from that of the grey substance of the brain, to that of the highest degree of lividity, and from the consistence of a scirrhus mass to absolute rottenness. In the worst cases it is difficult to say, whether the depth of the lesion or the quickness of its occurrence is the most striking.

In pursuing the dissection of these glands, when the crural arch and muscles of the abdomen have been removed, and the intestines drawn aside, then throughout the cavity a large quantity of blood will be found, which appears to have exuded in greatest quantity from the parts about the diseased glands; knotty tumours will be also seen extending in the course of the vessels, as far as the diaphragm, and this chain of enlarged lymphatics will be seen to have suffered considerable constriction, where it has passed through the femoral canal. All the glands exhibit a hard and united mass through the agency of the cellular tissue; here the circulation is impeded, and arteries, veins, and nerves, tangled in this lymphatic network, form a confused mass with effused blood. When cut through, the organization of the gland is seen to be altered from the natural condition in every possible degree, from the simplest subinflammatory state, to suppuration, and the subcutaneous cellular tissue, though more capable of enduring pressure than any other tissue, and therefore less often diseased, still in many cases passes into the same state of diseased change: the surrounding cellular tissue is generally healthy, and only in very bad cases found infiltrated. The common as well as the peculiar sheaths of the veins, arteries, and nerves, are usually speckled with petechiæ in those places where there is much motion: the lymphatic vessels do not appear in any instance to have followed the phases of disease in the glands, or to have become at all altered from their natural condition.

If the disease, in place of attacking the lymphatic ganglia

of the inferior extremities, and the abdomen, centralizes itself in the ganglia of the axillæ and of the breast, disturbance corresponding with the situation of the lesion arises. The same alteration takes place in veins, arteries, and nerves, and the same exudation of blood in the axilla and on the pleura along the course of the lymphatics, and of the thoracic ducts of either side. The case is similar in every respect, but that of position, to the former. In no case is the lymphatic system attacked at the same time in all parts. Thus a patient never has buboes in both axillæ and both groins, in both knees, and in the neck at the same time : and the respective ganglia of the thorax and abdomen, are never affected at the same time.

It often happens, that the conglomerate glands do not exhibit such changes, and that the effusion of blood does not take place. Then the whole disease is confined to the lymphatic system. In such case, the only thing to be remarked is, the greater or lesser swelling of the glands, and their grey colour like brain, undergoing a process of degeneration, even to decomposition. Then the vascular and nervous tissues which are entangled in the lymphatic network, do not exhibit the changes we have described, even when hæmorrhage is present, and the whole countenance has nothing of that lividity which is produced by congestion or stoppage of the circulation, and which occurs in these cases which we consider perfectly different.

Pulmonary Apparatus.—The diaphragm is almost always found healthy, it was only in two or three cases, that it was found slightly inflamed, and in others where this inflammatory appearance was absent, were seen some small petechiæ. The muscle itself betrayed no change. The lungs are crepitating, and less filled with blood than the spleen and liver. The bronchial mucous membrane has sometimes appeared slightly inflamed, but generally it is healthy.

Circulating System.—The pericardium is often filled with a bloody coloured fluid, most probably it is the usual secretion of its serous membrane tinged with blood. When this is the case, we remark tolerably numerous circumscribed petechiæ, sometimes on the serous coat of the pericardium, and sometimes under that of the heart. The heart is almost always one-third larger than natural : the orifice of the right ventricle and the ventricle itself, appear to to be dilated, and are always found distended with a quantity of black fluid blood, and often containing a mass of fibrine ; the muscular tissue of the heart is sometimes pale, sometimes manifestly softened, often natural.

The venous system is always congested, and the blood with which it is overloaded, is black fluid, and like bile. The

venæ cavæ, the subclavians, and the pulmonary veins, are frequently much enlarged, and fibrine is to be found in them, as well as in the sinuses of the dura mater, and cavities of the heart; besides this, these vessels, as we have remarked before, in the neighbourhood of those parts where the lymphatic glands are diseased, and where blood is effused, are much speckled and of a bluish red colour. Frequently on the surface of the blood in the large venous trunks, are to be seen little oily particles like those seen on soup. The arterial system is almost entirely empty. The arteries are normal, except where there is hæmorrhage, and then it is only on the external surfaces of their sheaths, that some livid spots are to be seen. The capillaries are empty.

Digestive Organs.—General softening of the membranes forming their coats. The peritoneum, the muscles, and the fibrous coat of the intestines tear with the slightest force. The stomach is often filled with a blackish fluid, the quantity of which is variable. Almost in all cases its mucous membrane is covered with much mucus, dotted with petechiæ, the colour and size of which differ, as do those of the skin, and which sometimes run together, so as to form a continuous bluish red surface of a very characteristic appearance, and which cannot be confounded with the inflammatory stage of acute gastro-enterite. In some cases the period at which reaction commenced, was marked by ulcerations situated in the depths of the folds of mucous membrane. These were confined to the thickness of the latter, and were sometimes two or three lines in extent, in other instances they were observed to follow the folds to the length of two or three inches: they occur in smaller numbers upon the other parts of the inner surface of the stomach. They always exhibit the black appearance of the state of gangrene in their centre, their circle is livid and tolerably defined. The small intestine is never collapsed nor dark coloured; generally its coats are not remarkably softened; its mucous membrane is covered with spots of a red colour, extended over a space of from five to six inches in length, more or less: it is often beset with petechiæ throughout its whole length, but they are of smaller size, and less confluent than in the stomach; sometimes these exhibit the appearance of little ecchymosed irregular streaks.

The mucus found in the stomach is to be found here also, in smaller quantity; the external surface, like that of the stomach, is of a whitish yellow. The ilio-cæcal valve is sometimes distended and livid, the valvular appendix is sometimes two or three times its usual size, livid-coloured, but generally natural. In the large intestine there is no remarkable change,

except that it is usually distended with gas, or half fluid greenish liquid. The black fluid found in the stomach, is rarely found in the intestine, which generally contains fluid of a bilious character. The glands of Peyer and of Brunner are natural.

Organs of Secretion.—There is nothing to be remarked in the liver, either in colour, size, or thickness; when sliced much black and thick blood flows out. The border of the left lobe has sometimes been found the seat of a small carbuncle, and the walls of the abdomen opposite to it were of a deep livid hue. Sometimes its surface is marked with small petechiæ. The gall bladder is often remarkably distended, and covered with bluish petechiæ. In twelve cases, its walls were many lines thick. The bile is generally in small quantity not thickened, and of a dark greenish yellow. The spleen is generally three or four times as large as usual; its serous coat is softened, and generally speckled with petechiæ, its parenchyma is generally filled with black blood of the colour of the lees of red wine, and its tissue much broken down, and softened to such a degree that it would fall to pieces were it not for the vessels and cellular tissue. The spleen was only found natural in four or five cases.

The pancreas, although generally normal, was sometimes found hypertrophied and hardened, yet without any change from its natural colour. The kidneys are generally enlarged from one-third to two or three times their natural size. There are frequently ecchymoses on their upper surfaces. When laid open by incision the cortical and tubular tissues appear filled with black blood, with which both pelvis and tubula are also filled, exhibiting the appearance of real hæmorrhage. There is often effusion of blood under the external coat of the ureter: its mucous membrane is always natural.

The bladder of its usual dimensions, is almost always natural; the urine is sometimes bloody. In some rare cases the mucous membrane is the seat of ecchymoses, of a bluish tinge, and like petechiæ in appearance.

In cases where hæmorrhage occurs in the iliac fossæ, the serous membrane appears as if there was effusion of blood under it, an appearance which is deceitful as it arises from the soaking of the effused blood through the tissues.

Having now drawn the picture both of the symptoms and the lesions, we would wish now to prove the relative worth of these two series of appearances, in order to draw from thence correct therapeutic deductions.

The number of lesions, and their intensity, is in direct ratio to the duration of the illness, and the results of its stages. Dur-

ing the period of prostration, there is a very manifest sinking in of the external buboes, without any general softening of the cellular tissue; there is little, if any, change in the digestive canal. General congestion in the venous system. No disturbance in the organs of secretion; the spleen is always found enlarged, and more or less softened. There is no bleeding from the kidneys. Greater or less enlargement of the lymphatic glands, with incipient softening, and even putrid decomposition; their colour at this period is never distinctly livid, but on the contrary there is often no trace of bloody effusion. This is saying enough of the manifest, perfect, extensive, and yet ever increasing lesions which this first period exhibits in the lymphatic and venous systems.

At the period of reaction, there are some slight pathognomonic changes; the winy appearance of the blood: the general softening of the cellular tissue; the increase in volume of the system of venous vessels; the state of decomposition of the lymphatic ganglia, and the exudation of blood around them; the occurrence of petechiæ both internally and externally; ulceration of the mucous membrane of the stomach; effusion of blood on the inner surface and on the circumference of organs. In endeavouring to account for all those, so many, so different, and such dangerous lesions as we have described, it is necessary to consider their situation, nature, manner of development, or succession to one another, their constancy, and their greater or lesser abundance, and thus to divide them into two distinct classes. 1st, Primary lesions; and 2d, Consecutive.

1st, To estimate these lesions it is necessary to cast a retrospective glance on the manner in which an individual may be attacked by the disease.

Whatever may be the ætiology of plague, whether its cause be infectious or endemic, accidental or epidemic, individual, local, or astronomical, were we even certain of it, such knowledge would not be of much value in the study of the disease. But when the influence of a pestilential cause is unknown, then it is of importance to shew what system of organs is invariably affected, and what part of this system suffers in the commencement. As the character of this influence can only be known by symptoms at the first outbreak of the disease, and by regarding the succession in which such symptoms occur, with the causes from which they spring, so it is evident that clinical observation will always be insufficient to give positive information with regard to its mechanism or first commencement, or to determine the first physio-pathological alteration which takes place in the organism of the part affected. But if we have recourse to some physiological data, to the anatomy, and to the patho-

logical chemistry, as well as to some facts in experimental medicine, we then can hope to prove that this affection can only be produced by absorption, and that the first diseased action occurs in the absorbent system.

Absorption of external matters must, taken collectively, be either by the venous absorbents in the lungs, or the lymphatic absorbents in the skin and mucous membranes.

Is the organism affected by the lungs? Is the infectious matter introduced into the circulation by the minute venous absorbents, after having penetrated into the vesicles terminating the bronchi, or does the pestilential cause act in such a manner on the pulmonary system as to alter the physiological phenomenon of hæmotosis? and lastly, does the blood, by one or other of these possibilities, acting on the respiratory organs, obtain diseased qualities? These hypotheses, which might be considered justifiable by physiology, are overturned both by the symptomatology and the pathological anatomy of the disease.

Can an injurious principle exercise its influence on the lungs without causing the least disturbance in the pulmonary system? Can the lungs be the seat of an abnormal process, without some portion of them, the tenderest, most susceptible or most irritable, suffering the slightest appreciable change in function or texture, and without a proportional series of pathognomonic signs appearing, which would give evidence of an affection of the lungs?

But even were the physiological theory possible, that the disease exciting principle could enter into the greater and lesser circulations, without exciting any change in the tissue of the lungs or bronchia, or affecting them at all, still, must not its presence in the blood have characteristic signs? Clinical observation more than proves that this is not the case by giving the histories of numerous patients whose symptoms were not alike, yet in the sum of whom the functions of the respiratory system were unimpaired, no irregularity existed in the contractions of the heart, none in the capillary circulation, no heat of skin, the digestive organs regular, and in whom the only decided symptom was lymphatic tumefaction.

Post mortem examination is not more favourable to this theory, inasmuch as it exhibits always a remarkable diseased change in the lymphatic ganglia, without this change being attributable physiologically to any change in the blood which had preceded it, or to any degeneration in the process of sanguification.

In those instances where the disease of the lymphatics is the only material and appreciable appearance, and in which

death occurs almost always suddenly, the blood certainly undergoes some change in its physiological properties, and this is probably the cause of death, by its reaction on the system of the great sympathetic, and upon the nervous system in general; but it is more probably only a consequence of what has previously occurred in the absorbent system, as we will endeavour shortly to prove.

When the disease of the lymphatic system advances, are the absorbent vessels of the skin on the one side, and those of the mucous membrane on the other, the means of its ingress? As science and observation cannot discover the primary material of pestilence, so they can attest nothing on its absorption. But let us examine to what point this absorption goes, which on retrospect is only acknowledgable from certain symptoms which it alone can explain, and by which the succession of the diseased appearances, and their pathological symptoms, can alone be accounted for; now as we cannot discover this unknown principle or influence, so we cannot positively assert its existence; but we may say that the means of exciting the disease has a double connexion, that of the individual, and that of the atmosphere. On the one hand, contact with the disease is necessary, but this on the other may be rendered powerless by the absence of a peculiar influence of season, without which the consequences of individual contact would remain concealed and inactive, and absorption could not be excited. Again, some think that the epochs of plague are governed by astronomical causes, and others that the destructiveness of contagion is quite evident, and if we unite these two chains of conditions with the cases observed during life, and the lesions remarked after death, we must feel convinced that plague can only be the contagious product of lymphatic absorption.

In the symptomatology the first appreciable signs are plainly pathognomonic of lymphatic absorption. For what is the only symptom which has been remarked at the commencement of the disease, alone and distinct from every other? Pain in the lymphatic glands, at first slightly throbbing, and as it proceeds growing more violent, and being more continued, succeeded by swelling and buboes.

As a pathological consideration, the change in the lymphatic glands is the only one which is constant, and at the same time the only symptom which is to be found totally isolated from all others.

What conclusion is to be drawn from these facts? That the lesions of the lymphatic system cannot be declared consecutive; their occurring alone, their constancy and essential pathognomonic symptoms render such opinion untenable. And if we

add to those particular conditions the reasons adduced for the infectious nature of the disease, and its production by absorption there will be no difficulty in recognizing the lesions of the lymphatic system as primary.

Another point remains to be explained; what may be the commencement of this lesion?

Does the matter of infection, after symptoms of absorption have appeared, act equally on the mouths of the absorbents, on the vessels themselves, or on the fluids contained in those vessels? When we carefully dissect the system of lymphatic vessels in those glandular tumours before mentioned, we find that there is no appreciable change either in those which pass to the gland, or those which leave it, but on the contrary, that they preserve all their usual appearances, pelucidity, elasticity, and strength; that the thoracic duct, and the lymphatic trunk of the right side are natural, that the vessels between the surface of absorption and the glands are never altered, and in no case share in the general softening of the healthy parts; that of the whole lymphatic system the glands are the only parts diseased.

Now, since the glands are always diseased, and the vessels never, it is evident that the disease is not conveyed by continuity of tissue, but that the diseased principle is introduced into the lymphatic circulation, and therefore the alteration of the lymph is sufficient cause and reason for the phenomena of diseased absorption, the pathological effects of which are displayed on the glands. The disease therefore arises from a change in the lymph.

Consecutive Lesions.—By the last work of M. Lippi, and the beautiful anatomical preparations of M. Amussat, it seems to be proved more than ever, that the union of the lymphatics with the veins, does not take place alone by means of the thoracic duct, but that these vessels besides this, are united by an innumerable quantity of extraordinary fine ramifications, and thus in different points, along the great veins, there is direct communication with the lymphatic system.

This discovery has necessarily enlarged the physiology, by which we can account anatomically for the quick passage of absorbed matters into the blood, by immediate venous absorption, better than by regarding it as the working of the great lymphatic circulation through the middle point of assimilation, the glands. If, in this condition of our knowledge, we reflect on the anatomical and physiological connexion of the lymphatic vessels to the venous system, with the originality and uncontrolled constancy of the diseased state of the lymphatic glands, on the one side, and on the other how the general symptoms of the numerous and quickly occurring lesions which post mortem

examinations exhibit, and which are most often developed at the period of reaction, we are led to the following conclusion—that the condition of general venous turgescence and enlargement, the proved presence of inflammable gases in the cellular tissue, in the veins of the head, foot, and abdomen, internal and external petechiæ, general softening of tissue, the enlargement, softening, and breaking down of the spleen, the spotted and ulcerated condition of the mucous membrane of the stomach, with the passive effusion of blood both within and without the organs, and the carbuncles, are lesions which follow a change which has taken place in the constitution of the blood. All the dissections which have been made prove that those lesions occur after those of the lymphatic glands have been established, and the necessary presence of the latter stands in direct opposition to the sometimes perfect absence, or the only partial presence of all or any of the others, so as to prove this fact beyond a doubt. But what we wish to establish is, that the different lesions which occur are not the product of an essential affection of the organ in which they are found, but that on the contrary they are only the accidental consequences of a general disease.

Three chains of facts may be adduced in favour of this opinion. The first arises from a summary of the symptoms, the second from the condition of the blood, and the third from the lesion itself.

During life, we have sought in vain, for pathogomonic indications in the symptoms: after death we have compared them with the lesions in the dead body, and it has appeared to us that they were not capable of giving, as signs, explanation of an original local affection, either individually or collectively; but on the contrary, they manifest themselves as results of some other local change which has occurred, as for instance, that of the general affection of the lymphatic glands.

The general nervous symptoms, shivering, headach, disturbance of the cerebral functions, quickness or smallness of pulse, frequency of respiration, vomiting, petechiæ, carbuncles, and buboes, are neither characteristic of a vascular nor of a nervous inflammation, neither of an idiopathic gastro-enterite, nor of an essential inflammatory fever. Throughout, they contradict every thought of vascular inflammation, neuritis, angiocarditis, or, in a word, any affection of the solids. Doctor B. then proceeds to prove that the want of accordance between the lesions and symptoms of plague renders it impossible to compare it or to confound it with gastritis, gastro-enterite, hepatitis, inflammations of the kidneys, breasts, brain, or its membranes, all of which are marked by the occurrence of their own peculiar symptoms. Besides there is one circumstance which is very

remarkably opposed to this, that in the greater number of cases, where symptoms indicative of all those diseases were remarked, neither in the stomach, spleen, kidneys, nor brain, was any lesion found; in short, one only of two possibilities can be admitted, either that the symptoms are the impression of an idiopathic disturbance of the functions, without any possible organic lesion or known material origin, or, they are the signs of a change in the blood.

Against the theory of an essential disturbance of the nervous system, numerous cases of plague may be adduced, where the organism pursued its normal course, and where the disease of the glands appeared, continued, and ceased, perfectly separated from, and unconnected with any thing else. 2ndly, the whole of the great sympathetic nerve was examined in twenty cases, and in all it appeared perfectly free from disease both in trunks and branches. 3rdly, this system has lost none of its peculiar characteristics, whilst the functions of life, which depend upon it, are nearly merged in death. The irritability of touch, sense, and hunger, were evident, and as the power of the nervous system consists in an union of capabilities to produce irritation, to perceive and to move, so it is evident that it can in no case be injured without its functions being injured also. Shall we then, in such unity in the functions of nervous life, imagine that there is a primary alteration in the nervous irritability? How can this be explained theoretically? Is it by the action of physical agents, which can only alter the nervous centres by contact? Physiology sets aside this possibility. Or is it by the agency of fluids which mingle with, and are assimilated with the primary elements of the nervous system, or in other words, by organic bodies which, by the force of their selective powers, exercise on the nerves one of the two known influences, paralysis or inflammation. But here, on the one hand, we could not comprehend what these bodies might be, and even did we succeed in discovering their existance, still we should be forced to ask for proofs of their mechanism and power of action. In short we are either mistaken in the function of the sympathetic nerve, or some of the lesions of which it is capable have escaped our researches.

We come now to the second hypothesis, a change in the blood. Blood may be examined comparatively in this as in other diseases. In no stage of this complaint, does the blood, no matter how it may have been drawn, exhibit the inflammatory, buffy coat, as is seen in blood drawn in acute inflammations. From this it follows that plague is not an inflammation, either essential or secondary, and that, therefore, the consecutive organic changes throughout, cannot be regarded as the

effects of inflammation. In every venesection the blood flows easily and in continuous stream from the puncture ; its consistence is, however, commonly greater than in a state of health : its colour is dark red, and it continues so till the bleeding is finished, without ever exhibiting the appearance of a change into bright red, so common in bleeding in other complaints ; it has sometimes a peculiar odour ; when left at rest, it assumes, after an hour, a violet red colour ; in the generality of cases the blood is imperfectly cupped, like curd, over which a red-coloured serum swims ; sometimes the whole mass remains fluid, and no coagulum is formed, and then its colour is livid, its odour strong, and oily drops swim on its surface. In the examinations made after death, the blood in the arteries was always found as black as in the veins, but in very small quantity ; it was always fluid, dark, and seemingly decomposed. In many cases, where the great venous trunks were opened, drops, as if of oil, were observed on the surface of the blood, similar in appearance to those seen on the blood drawn by venesection. Some analyses have been made by a French chemist, M. Rochet, which we intend publishing in a forthcoming work. We have made similar experiments to those made by M. Rochet, and are able to confirm them. The blood in plague contains very nearly the same variable quantity of albuminous serum, fibrine, colouring matter, and water, as in the healthy condition. Reagents constantly exhibit the presence of a free gas ; the serum is often tinged red ; the clot is always more or less imperfectly formed, and of much less firm consistence than in health ; and that a fatty matter is to be observed on its surface like the oily particles on soup.

Thus the blood drawn by venesection, and that of the dead body, constantly exhibit signs of a remarkable change, and as the collection of the symptoms and lesions does not speak pathognomonically in favour of a local or general inflammation, so the blood also shews no evidence of any such affections. Having determined the pathological condition of this fluid, it remains to inquire into its necessary and genuine influence on the disease. Does this depraved state of the blood exist during life with all the symptoms, or does it precede them ? is it only the consequence of decomposition which first commenced with death ? is it vital, or cadaveric ? the cause or the effect of death ?

We possess a great many proofs of a remarkably depraved state of the blood. In five cases, after twenty hours' illness, and ten hours after death, the whole cellular tissue and the great venous trunks were inflated and distended by gas, which on bringing a light near it, burned with a clear flame ; in three other cases, after forty-eight hours' illness, and twenty hours after death, the system of venous vessels, were all of a dark,

livid hue. In those curious cases, in which the pathological changes bear no proportion, either to the duration of the disease, or the time elapsed since death, it is quite impossible to attribute it to the mere results of cadaveric putrefaction, or a decomposing fermentation. The alteration of the blood, far from being confined to the intestines, is general; from the capillaries to the veins of the lungs, the same irregularity is observed, the same lesions, the same change in fluids and solids, the colour which pervades their tissues, black fluid blood, and some molecules of fatty matter. We cannot conceive any decomposition of the body after death causing this colouring of the vessels, because it is too equally diffused and too general: the redness also is too strong and striking, not to compel us to attribute it to organic action, not of the vessels alone, but of the whole system, thus to diffuse over all parts a fluid having equally destructive qualities. It is just as difficult to prove the antecedence of a process of decomposition, because it is impossible to conceive that one and the same fluid, in so many different circumstances of form, structure, capacity, and texture of the different viscera or organs, should always exercise the same kind of alteration. The decomposition of the blood is not deducible from cadaveric putridity, because it occurs before death, and we feel little doubt in considering it as the cause of death, as well as of all those lesions which have occurred during life.

Since this decomposition of the blood during life is acknowledged, we might say that this perfectly vital change might remain also after death, but on the contrary we would willingly believe, that immediately after the extinction of life, the laws which govern inanimate matter, and those which have caused this diseased state of the blood, must immediately change places, and that the vital decomposition must quickly yield to the succeeding cadaveric. The state of our knowledge gives no very striking proofs of this, but still it may be received as probable.

If it be true, then, that the blood does not preserve its physiological characters, that its elements for the support of life are changed, what then is the cause of this change—what is its character, and in what period of the disease does it commence? We have two things to regard here, the inflammation of the lymphatic glands, and the diseased pus, their product. When the inflammation is abortive and disappears, then the affection of the glands yields and dissipates, and the attacks cease. If it continues, on the contrary, then all the lymphatic glands wherever the disease passes, are filled with a grumous and diseased discharge, long before the formation of pus: this happens agreeably to the laws by which the lymphatic circulation is carried

into the general, where it soon shews its presence in certain organs, and certain parts of the organization, by producing those severe numerous and rapid symptoms before enumerated. The blood is decomposed also by the more or less advanced degeneration, the seat of which is in the lymphatic system; as soon as the diseased lymph has entered the venous circulation, it loses its normal qualities, and causes a general disturbance, a deep disorganization in all its functions, in short all the derangements of true poisoning. What has proceeded in the progress of the disease, exhibits exactly what Dance has described in his work on Phlebitis: "the matter sometimes mingles itself with the blood by entering into the circulation, it shews its presence there by causing disturbance, which can only be ascribed to the one cause; in general after violent and continued shivering, a number of severe symptoms are seen to arise suddenly, which militate against the vital principle, and indicate the existence of a destructive influence in the body, viz. a sudden change in the expression of the countenance, an extreme feeling of weakness, an undetermined irregular delirium, perfect indifference; later a dry sooty tongue, imperfect articulation, difficult breathing, jaundice, &c. At a later period, the patients fall into that kind of dangerous fever, which with great uncertainty is regarded as dependent on the violence and extent of venous inflammation; there are spasms of the lips, and sometimes petechiæ suddenly appear on the skin, inflammation of the parotid quickly occurs, or gangrene, then sub-sultus tendinum, the pulse is frequent and compressible, and death soon occurs." Similar appearances are remarked in all cases where morbid matter, and the excitants of morbid collections, are absorbed into the economy. Here there is only an artificially induced change in the fluids, its nature is easily determined, and we can readily estimate the symptoms and lesions which are developed under its influence; for observations of this kind, to be of any value, must be always particularly accurate and true. It is quite different in the disease produced by morbid alteration in the fluids: in it the diagnosis is always dark and uncertain, and it is impossible to make a sure prognosis; even by examination after death, there remains often great confusion as to the real nature, causes, and seat of the disease. All the viscera of importance are diseased in a greater or less degree, and from the spirit of the schools, and long rooted theories, one feels always disposed to estimate and deduce all these disturbances from purely local and organic affections.

The general colouring of the blood-vessels favours our view of this matter: we have before mentioned, that the whole system of venous vessels is reddened, without either thickening

or weakening of its tissues, except where fibrinous masses exist in the pulmonary and iliac veins two or three inches in length, and in these situations the vein is not reddened. The deduction to be drawn from this is, not that the redness is a consequence of inflammation, but that of a peculiar change in the blood. According to the experiments of Trousseau and Rigot, it has been established that colouring in the dead body can be produced with incredible quickness, when the blood possesses certain qualities. Here, where the blood is always found livid, and apparently decomposed, the stomach of a lead colour, its mucous membrane swollen, the spleen softened and filled with black and thin blood, the heart and liver soft and discoloured, will not any one in these signs of derangement recognize the symptoms which proceed from a decomposition of the blood?

If, on the one hand, the powers of venous absorption be not denied, and the extraordinary permeability of their tissue during life be considered, then the possibility of a transudation of blood through the vascular tissue may be granted. Thus by the assumption of the simple phenomenon of vital imbibition, which is much favoured by the constantly enlarged state of the veins and capillaries, not only this distention of the vessels, but also the cause of the petechiæ, ecchymoses, and exudations of blood, might be explained.

Although this is but hypothesis, still it appears preferable to the assumption of acute inflammation, to which both the symptomatology and the pathological condition of the blood, as well as that of the lining membrane of the veins are distinctly opposed. But if a change in the vital properties of the blood, and in its organic composition be admissable, then I should say that I consider the first change to take place at a very early period of the disease, or with its first appearance.

From this period the blood having received either directly or indirectly the diseased matter from the lymphatics, loses its physiological character, and assumes a perfectly peculiar one, only to be accounted for by such a cause.

This change in the blood may also be attributed to the black stuff found in the stomach; this is adduced from the chemical components of this matter, amongst which the oxyde of iron, albumen, a slimy and a colouring matter, &c., are found, as well as from the weakness and distention of the capillary vessels, and the chemical nature of their contents. In this morbid condition a secretion comes from the walls of the stomach, the capillary vessels permit a transudation of blood to take place through their enlarged pores, which in lieu of undergoing the change which it would in a sound condition of the organ, comes into a diseased stomach where it can be no longer organized.

Notwithstanding those reasons, we would wish to seek how far those lesions remarked after death are accordant with clinical facts, as to the admission or rejection of an essential inflammation. We have stated already, that in many autopsies there were no appreciable lesions, and, therefore, no cause of death found. M. Broussais is of opinion, that in those cases which were tolerably numerous, the inflammation had not time to develope itself, and that death arose from an affection of the nerves. But what becomes of this acute gastro-enterite of which he conceives plague to consist? To pass sentence on so extraordinary an impression, it is only necessary to open a single subject which had died of plague, when its impossibility will at once be seen, and a rebuke given to the dogmatic assertions which have been made on typhus, yellow fever, and plague.

Of all the concomitant alterations in plague, that of the spleen occurs most frequently; it is rare to find this organ sound in five cases in a hundred. It is very difficult to understand the anatomical pathology of this circumstance, if it be not sought in decomposition of the blood. It is difficult to think of a peculiar change, of an inflammation, *sui generis*, of an essentially idiopathic splenitis, for if the essence of the disease rested in this affection, then it would always occur, but as this is not the case, it is easier to attribute it to the preceding effects of a pathological cause.

Next to the spleen, the intestinal canal, and particularly the stomach, are the parts most frequently the seat of lesion, yet this cannot be taken as a necessary concomitant of plague, as it is not found in more than eighty bodies in the hundred, or it is so very trifling, that comparing it with the deadly symptoms, it must appear unimportant.

Plague then can exist unconnected with any fortuitous change in the intestinal canal, and cannot, consequently, be regarded as a gastro-enterite of a peculiar character; the diseased condition whence it springs, is not of the same kind, and has nothing in common with gastritis. In the greater number of cases the affection of the stomach is limited to the simple appearance of petechiæ, similar to those on the skin, and it never has occurred to any one to consider the latter as arising from inflammation of the skin. In both cases the petechiæ can only be considered as the symptoms of a passive hæmorrhage, which careful dissection will abundantly prove.

Far too much weight has been laid on the pathological condition of the stomach and intestines by those who have regarded them as primarily affected. Their alteration is only an accidental and consecutive affection, like that of the spleen. In many cases where these petechiæ become confluent, and exhibit a livid

extent of surface, it might be mistaken for intense inflammation ; by a close examination, however, the original petechiæ are discoverable. In the second stage they may also assume the appearance of an inflammatory character, when they ulcerate in the direction of the folds formed by the mucous membrane, but this is no argument against their being essentially passive and consecutive.

Increase of volume, and a slight change in colour and thickness, are the only changes which take place in the liver. The rareness of their occurrence and their unimportance, sufficiently denote their trifling influence on the course of the disease.

Congestion in the lungs is rare, and inflammation in the bronchi still more so, and yet a pathologist who had never seen plague, boldly announced this inflammation as its essential cause. But although the pathological condition of the respiratory apparatus is simply congestive, still, in many persons, the lungs are found perfectly healthy ; from this may be judged the little influence which they exercise on the course of the disease. Congestion in the whole system of venous vessels is an important condition, and one which we have remarked in all cases ; all the tissues are filled with dark venous blood. If an incision be made into the brain, lungs, liver, spleen, kidneys, heart, or large vessels, quantities of black venous blood flow out, as if under the pressure of some mechanical cause.

How is this appearance to be explained ? Is the cause to be attributed to stagnation in the capillary circulation, or to some obstacle in the organs of circulation, caused, perhaps, by the enlargement of the lymphatic glands, through which the vessels must pass, or to a general inflammation of tissue ? Or is this state of congestion, this kind of venous apoplexy, the regular consequence of an alteration of the blood which will not circulate, and of a superfluity of this fluid in the subjects of plague, as plethora always appears as a symptom of this disease ? We found it in the majority of 3000 cases which we examined, and its pathological effects were much favoured by the constriction of the capillary circulation. Besides, this theory receives double attestation from the occurrence of petechiæ, and the increased transudation from the skin.

It is also proved that neither the examinations after death, nor the symptoms and condition of the blood, justify the application of the physiology of Broussais to plague, but on the contrary, they shew that those organic affections are accidental, and that the disease can exist without them ; recovery also proves it, and it is unreasonable to suppose that those lesions always took place, when they are not to be found in the majority of bodies examined. Therefore each local affection can be considered

only as consecutive to the disorder of the lymphatic system, in which the simple original affection, the essence of the disease consists, and without which no general disturbance would have occurred.

Treatment.—As the course of research hitherto represents the pathological appearances, only as types of a general affection, in which the solids are only secondarily affected, so now the question of treatment arises, grounded on clinical observation, and individual experiments.

We will confess, that of the whole medical history of plague, the treatment is the part most in the back ground, and the part as yet most imperfectly explained; it is easily conceivable how difficult it must be to adopt a proper practice, when we consider the number of cases, their intensity, quick course, and the uncertain diagnosis from symptoms, as well as the general nature of the lesions.

Here Dr. B. introduces much apologetic matter for bringing forward this part of his subject, which he considers as yet but imperfectly investigated; we must omit it, and hasten to the conclusions on which treatment is to be grounded.

1st. That plague is a contagious disease, whose specific and purely individual causes can only be rendered effective by the contemporaneousness of meteorological phenomena.

2nd. That it is impossible to judge of the value of symptoms, as they afford no certain clue to the situation and essence of the disease.

3rd. That the number and importance of the lesions is in direct ratio to the length of the illness, and the change which occurs between its stages.

4th. That those lesions of the first stage which denote disturbance, are more or less remarkable development of the lymphatic glands, with softening or a putrid induration, often without lividity, and venous congestion, with or without a slight enlargement of the large vessels.

5th. That those of the second stage are, first, a decomposed state of the lymphatic glands, with effusion of blood; second, general softening of tissue; third, a remarkable state of congestion; fourth, internal and external petechiæ; fifth, ulcerations in the mucous membranes of the alimentary canal; sixth, ecchymosis; seventh, carbuncles.

6th. From the manner of their development or order of succession, all changes are to be divided into primitive and consecutive.

7th. The isolated condition, the constancy and the pathognomy of the lymphatic disease, and the impossibility of considering it a consecutive affection, as well as the reasons adduced

from the contagious nature of plague, prove that this lesion is essential.

8th. The lymphatic vessels entering and quitting glands, are in their natural state, as well as the ducts in the thorax, the glands alone are more or less constantly altered.

9th. The lymphatic vessels are never altered, the glands always; this follows necessarily from their relation to the animalization of which they are the seat, and the glands become diseased by having to exercise their powers against a morbid absorption which must be carried to them, of diseased lymph or a fluid-like lymph.

10th. Every thing adduced with regard to the original lesion of the lymphatic ganglia, is also favourable to the opinion of the lymph being diseased.

11th. That the inflammable gas, and the condition of distention and enlargement, often remarked in the largest venous trunks, &c., the general softening of tissue, the internal and external petechiæ, the decomposed spleen, the spotted and ulcerated condition of the stomach, the ecchymoses, effusions of blood, and boils, are all consequences of a diseased state of the blood.

12th. That these lesions do not arise from any special affection of the organs in which they are found: they are only fortuitous consequences, complications, and accompaniments of a general disease, which often occurs, and proves fatal without them.

13th. That the general affection is not an inflammation of the vascular system, for in many bodies no trace of it was to be found, and in others the disturbance was so trivial that it could not be considered as a cause of death.

14th. That the blood never exhibits the inflammatory coat; and the symptoms are never such as to be pathognomonic of the existing lesions.

15th. The symptoms, the condition of the blood, and the examination of bodies, are contrary to every representation of inflammation of the vessels or local phlegmasia; but they justify, in a remarkable manner, the theory of this affection proceeding from a morbid state of the blood, succeeding on its part to an affection of the lymphatic ganglia.

Let us now seek the therapeutic indications arising from these data, and analyse the attempts made to fulfil them.

In the year 1834, when the great Egyptian plague raged, we commenced our observations. As some of us had never seen the disease, it was natural that we should not lay down any fixed plan of treatment, till from clinical and post mortem observation we had derived some knowledge of the seat and

nature of the malady. This, however, was not the case, and we were obliged to confess that the symptomatology and pathology, the sick and the dead, were equally silent in giving us information with regard to the treatment.

After five months experimenting with all kinds of treatment, and all modifications of it, adding to proofs and specifics of all kinds, and about 1000 instructions, at last we arrived at the melancholy negative conclusion, *that the medicines produced their effects upon the organization, but that the malady neither ceased nor changed.* Such were our feelings when we arrived at Smyrna, where for one month and a half we practised, shut up with the sufferers, an half instinctive, half empirical symptomatic treatment, grounded on the knowledge we had acquired of the lesions which had occurred.

On the admission of those patients who exhibited the characteristic symptoms and state of prostration of the first stage, and when it was but from six to twelve hours since the seizure, we administered stimulating and diffusible medicines in increased doses, as sal ammoniæ, ammoniac, iodine, liq. chlorinii, nitric acid, alcoholic tinctures, and preparations of ether.—These various preparations did not appear in any case to act forcibly on the nervous system. Sometimes it appeared as if the urinary and cuticular secretions were about to be restored; the stimuli for a moment appeared to produce a change in the expression of the countenance, and to act on the muscular system, but all the sick died after some sparks of vitality, excited with difficulty, had appeared, in 24, 36, or 48 hours after their admission. Under similar circumstances of time and symptoms, in order if possible to hit on some untried method of preventing or obviating the attack of the bowels, we gave emetics in gradually increased doses at intervals. We always obtained a double effect, vomiting and perspiration. Along with the fluid of the emetic was vomited a slimy or bilious fluid, and the face and body was covered with a clammy perspiration, but the limbs remained dry. In no case had this means any useful result, or the slightest influence on the favourable or unfavourable termination of the disease, and the circulation and breathing were never excited by it.

Contrary to the usual indications in nervous symptoms we employed, in the same stage, narcotics, in order to moderate the violence of the circulation, to lessen the disturbance in it, and prevent vomiting; the Tinct. Thebaicæ, Ext. Opii, Hyosciami and Thrifax (Lactucarium) caused perspiration, had same effects on the circulation, and caused the vomiting to cease; but the regular course of the disease was as little altered as if nothing had been given.

Induced by the rigors which the majority of patients assured us they had felt at the commencement, we tried the Sulphate of quina, but it produced no beneficial effects.

Twenty sick persons were treated with $\frac{1}{8}$ gr. of strychnine hourly; subsultus tendinum occurred in all, without any other evidence or symptoms of its effects.

Calomel was always thrown up by the vomiting, or evacuated by the watery motions which it caused.

Supported by the observations of M. Velpeau, on the application of mercury in acute peritonitis, and on the known action of this remedy on the lymphatic glands, we tried mercurial frictions and blue pill. The effects were such that it remained doubtful whether the patients so treated suffered from disease produced by the medicine, or from some new phase of the disease itself which might have occurred without either the inunction or pills. Regarding our own convictions alone, we are not inclined to put much faith in this remedy.

So much for treatment in the first stage; when the malady reached the second stage, if the powers of life overcame the disease, then there arose a kind of momentary reaction, which could not be compared with the state of inflammatory reaction which arises from the adynamic stage of many fevers; despite of the return of pulse and colour, injection of the eyes, dilatation of the pupils, and the condition of the tongue, the blood never displays an inflammatory coat, bleeding and antiphlogistic measures exercise no apparent effect except that the reaction is impeded, and the result rendered more certainly fatal by their employment.

In this stage new diseases (if we may say so) are developed in as great number as there are organs. Each viscus becomes the seat of a perfectly passive congestion; passive with regard to the organ where it exists, for abstraction of blood causes the torpor of the intellectual powers to continue, and the general sinking to increase. General antiphlogistics are more dangerous than useful. In this second stage death is always caused by a condition of congestion, stagnation, or turgescence in the veins, or by the affection of the organs themselves, or by absence of crisis, or even when this occurs, and the patient throws off the general affection, by means of the affection of the glands, which pursues its course to suppuration, unconnected with any pathological relations in the organs, which is proved on opening the body by large collections of matter being found under the pleura and between the folds of the mesentery.

We have often seen the vital principle alone, without any critical phenomena, subdue the general affection. The important symptoms vanish, the patient becomes better, and then sud-

denly dies. In these cases there were always large formations of matter.

After what has been stated, we cannot regard those Physicians who pretend to have cured this disease with leeches and cold water, as much better than dreamers, or deceived; or perhaps their remedies were applied at the period of reaction, when the disease would actually get well without them, and drew the same credit from their supposed nostrums, that the homœopathsists, or, in other words, the practisers of expectant medicine have done from theirs; less fortunate than those dogmatists, we remained for a moment in the same errors, and then, as quick as possible, followed our consciences.

After so many abortive trials, we adopted the principle of vital action for our new belief, and determined to make it the ladder of our new experiments.

From the history of cases, it appears that there is a turning point, at which the powers of life make an effort at recovery, and when unaided, they repel the morbid principle and triumph over the derangement it has caused. This is what we have called the critical period. This kind of sustained orgasm, and the instinctive feelings of the patient, form the basis of the natural treatment which we ultimately have adopted. On one hand, we have the healing powers of nature manifested of their own accord by a general relaxation, and the sudden breaking out of carbuncles on the skin, active suppuration in the buboes, and effusions under the skin, or bleeding; on the other hand, the instinctive feeling prompting the refusal of food or medicine of all kinds. So long as the sick can make themselves intelligible, they answer all questions put to them, by demands for water; when the critical period is about to appear, they give their assistance to support and hasten it in every way. Should perspiration appear, they desire a warmer temperature, and cover themselves under the pillows and ask for more bed-clothes, to escape from all contact with the air; if at this period cold water be offered them, they either refuse to take it, unless warmed, or drink it unwillingly. If carbuncles appear, they shew that they are aware of their healthy influence by watching them themselves, and directing the attention of their physician to them. If the crisis concentrates in the buboes, then they are every moment anxious that they should be cut out. Should the hæmorrhagic form appear, far from being terrified at its intensity, the only thing they dread is its too quick suppression, or that the throbbing pain which precedes the irregular eruption on the skin, may drive it away.

This is the natural history of the disease when let to run its own course, and such are the natural feelings of the patients;

and these are the general rules by which the double powers of instinct and vitality can be used effectually as remedies. By discovering thus the real termination of plague, and by attentively remarking individual and pathological conditions, we were led, first, to give up all attempts at cure by medicines, and next, to wait quietly, to imitate nature, and to call forth her powers, when the vital principle, of itself, seemed to be powerless, obstructed, or sluggish.

Treatment is only useful when employed in the first stage of the disease, on the first, second, and in some rare instances, on the third day after the seizure. Beyond that period, if nature cannot effect a cure, the disturbance becomes general and the patient sinks.

In the commencement of the stage of prostration we have given every quarter or half hour two or three spoons full of warm lemonade, every hour four to six drops of tinct. thebaic., or every two hours four or five grains of Dover's powder; every three hours rubbed in two drachms of strong mercurial ointment on the abdomen and inner surface of the thighs. Applied poultices to the buboes till they spread, then made large incisions in them which were simply dressed; when carbuncles appeared they were punctured and dressed with a latticed compress, till the slough came away, or was easily removed. When under the influence of the first of these remedies, abundant and continued perspiration occurred, then the opiates and the mercurial inunction were omitted, and the patient recovered. This was a rare result, however, for the disease generally hurried on to the second stage, and besides the greater number of those affected first entered the hospital on the fifth or sixth day after the disease had appeared. When, even at this period, no eruption of boils with broad surfaces appeared, then two or three tumours, of a similar kind, were artificially produced, by the introduction of four, six to twelve grains of sublimate into the subcuticular cellular tissue in the neighbourhood of the lymphatic ganglia. If external buboes did not exist, they were artificially produced by deep incisions into the glands of the groins and axillæ, and by the introduction of irritating and caustic lotions into the bottom of the wound, and immediately closing it, by the first intention.

By this new plan of treatment, the disease was almost always concentrated in the glands of the axillæ and groins, and an extensive discharge produced. The general symptoms arising from a local discharge, and from a partial absorption of mercury, developed itself; the breathing and circulation assumed a new rhythm; there was general excitement produced; the skin began to perspire, and the peculiar diseased action of the system under-

went a change, and the sick recovered quickly, under this physiologo-therapeutic influence.

By the application of this method, so energetic and quick in its effects, our views for sustaining life were much renovated, and a great number of sick were cured. The only unfortunate circumstance was, that the sick came into the hospital in such numbers, and in such quick succession, that we were unable to treat them all, and when we saw them first they were generally half dead. Amongst more than 200 sick, there were not thirty whose condition warranted an attempt at cure. Such an unfavourable condition must greatly affect success, yet we have convinced ourselves that even in such cases unexpected results may occur.

Reaction in plague is not unlike secondary typhus, and its therapeutics have decided characteristics. In general it is better to follow an expectant practice, and wait for the action of the vital principle at the critical period, and not to set it astray, weaken the vital powers, and thus obstruct recovery by active treatment. We would confine ourselves to the administration of some gentle diaphoretic drinks, or of some excitants of the excretive and secretive functions. The treatment of the crisis should be that which the symptoms demand.

Conclusion.—Experimental Medicine.—We will now give the experiments which we have already made, or intend making, in order to determine the principle which generates the disease, and to fix a character on the pathological theory which we have given.

Doctor B. enters into a lengthened disquisition on the police history of plague, in which he declares that all the quarantine regulations which have been established, were grounded alone on theory, and that it has been a doubt always, whether they were ineffectual or too strictly enforced; in fact, that they were an empirical experiment, yet one from which very little can be deduced. He then asks, even with the strict regulations at present enforced, whether any one could say, that in a year, a month, an hour, the plague might not produce in France a repetition of 1721, or in England of 1665; that it may not repeat its frightful ravages in Mailand or Florence, in Moscow or Athens; and although experience should answer all those questions favourably from the fact, yet even then it would be impossible to be assured of the future, so long as a perfect ignorance of the real essence of plague, compels lawgivers and physicians to act on theory.

To discover this specific cause or essence of plague, numerous experiments have as yet vainly been made. The commission of which we formed a part at Cairo in 1835, conducted

some of those experiments without removing any of the difficulties, because it committed the same errors which had been made by others, and left the same hiatuses to be filled up, so that science gained nothing, and all its reproaches remained.

The sum of experiments and observations made to prove the contagious nature of plague, may be separated into three divisions. The first comprehends the facts of immediate contagion; second, of mediate contagion; third, those derivable from inoculation.

First.—Immediate Contagion.—In order not to encounter the positive proofs, and force ourselves from the direct path which we have laid down, we will only give our attestation to those of which we have ourselves been witnesses. These require no comments to render them absolute proofs of immediate infection, and are valuable from their shewing how this means of propagating the disease may be avoided. But as these proofs are not always seen under the same appreciable conditions, and as the causes of their occurrence are not better known than those which prevent them, the anti-contagonists deny the operation of a specific agent, and the consequences of individual contact, by supporting their arguments on many negative cases, as if this infection were less probable than that of syphilis or scabies, epidemic influence, or inoculation. These negative conclusions are manifestly specious, and can have no validity, for independent of the facts which we have adduced, we can positively assert that those places where a true quarantine has been observed, although situated in the midst of infected places, are preserved from the disease, and enjoy a perfect immunity from their isolation.

In fact, in Egypt as well as in Smyrna, a great number of military institutions, schools, &c., and ninety-nine out of a hundred private dwellings, where strict quarantine was observed, were never attacked. Let any one closely examine our reasons, and they will find that the facts on which they are grounded admit of no other explanation.

Second Division.—Mediate Contagion.—On the 15th May, 1835, at nine o'clock A. M., I clothed myself in the plague ward of the Hospital Ezebekieh, in presence of all its officials, and without having employed the slightest previous precaution or preservative, with the shirt of one who had died of plague of the very worst form. This shirt was still warm with the heat of the patient, and bloody from a venesection which had been just performed. Throughout the entire day I remained in company with the witnesses of this experiment, in order to have evidence that I employed no means to prevent absorption, in case such should occur. I wore this shirt forty-eight hours,

I had no general symptom, and did not experience any thing which could reasonably be ascribed to these clothes. For although two days afterwards a boil broke out on the middle finger of my right hand, yet I regarded this rather as the consequence of a slight scratch which I had received in dissecting a person who died of plague, and on whose body were two carbuncles.

2nd. On the 17th of August, at eight p. m. the criminal Ibrahim Hassan, eighteen years old, was clothed with the shirt, vest, and trowsers of a man badly affected with plague, and then placed in the yet warm bed of the latter. Up to the 21st no sign of absorption, or of the development of any symptom of the disease showed itself; but on the evening of this day he complained of slight headach; prostration commenced; violent headach; dilated pupils; white moist tongue; tone of voice unchanged; but his manner of answering slow and tremulous; breathing frequent; skin hot and dry; pulse hard, 120 to 130; there was neither vomiting nor diarrhœa. Early on the 23rd a bubo appeared in the left axilla; in other respects he was as yesterday, without being manifestly worse; violent thirst. 24th. Bubo remarkably developed, and very painful throbs when he moves; the skin over it not discoloured; abundant greenish matter vomited; diminished frequency, but smallness of pulse; less disturbance of the breathing; a kind of coma, with anxiety similar to a condition of narcosis. 25th. Vomiting continues of a deeper green colour; tongue dry and somewhat sooty; pulse very small and frequent; breathing quick and short; less prostration; slight excitement of countenance; death in the night.

3rd. On the 7th of August, at eight p. m. Mohamet Ebn Ali, a condemned criminal, was clothed with the shirt, trowsers, and vest of a person violently affected with plague, and placed in his bed. Nothing extraordinary happened till the 22nd or early on the 23rd, when the disease commenced. His gait became staggering and difficult; there was excessive prostration; countenance deeply downcast; eyes shut; pupils dilated; violent headach; white moist tongue; complaining tone of voice; frequent sighing; skin warm and dry; pulse small, 120 to 130; neither vomiting, diarrhœa, nor delirium. 24th. Abundant bleeding from the nose for two hours, which ceased of itself; a bubo appeared in the left axilla; then came the stationary condition. 25th. Some sleep in the night; decrease in the intensity of all symptoms; pulse fuller and less frequent; tongue moist, broad, and almost natural. 26th. The bubo opened; convalescence commenced.

By adding these facts to those remarked before, we then have proofs of all kinds of mediate contagion, and this will

serve as an *a priori* argument to prove individual contagion, for we cannot understand how the cloths could communicate contagion, when the patients themselves could not.

Doctor B. adverts in no measured terms to the fault committed by the Cairo commissioners in these experiments, and we think with reason, for they were perfectly useless in attempting to establish the fact of mediate contagion, because the patients were exposed to all the other causes which might generate the disease, as well as that derivable from the clothes, from the circumstance of their being in the wards, and surrounded by the plague patients. The experiments to give positive evidence should have been instituted on persons perfectly free from disease, and completely isolated from it except in the particular of the clothes.

Third Division.—Inoculation.—The matter of buboes, portions of glands in different degrees of decomposition, blood drawn from the veins, heart, and great vessels of dead bodies, serum from carbuncles, and carbuncles themselves, were inserted in little pockets made in the subcutaneous cellular tissue on the inner side of the thighs, neck, belly, back, &c., of many dogs. Being watched for ten days, these animals shewed no trace of suffering, their general condition was not at all altered, the incisions quickly healed, there was nothing striking in the local inflammations, except that they were generally proportioned to the size or superficiality of the wound.

The bloody serum from the head of a boil was introduced into the cellular tissue on the inner side of the leg of an ass: no sign of absorption manifested itself. The same pathological product eaten by dogs, had no positive consequences, and the animals remained in health.

These negative experiments tempted to a repetition of them on criminals, in order to learn whether the morbid matter was really harmless when inoculated, or if it proceeded from difference of organization. On the 18th of August, we inoculated a criminal, named Hassan, by means of four incisions in the fold of the right arm, with blood from the cephalic vein of a plague subject who had been two days ill. The following symptoms appeared: white and moist tongue, dry skin, pulse 115 to 120, vomiting, buboes in the left axilla, petechiæ. Up to the 20th, there was not the least disturbance; the following night, however, the patient complained of general illness, and on the 21st, at 7, A.M., the following symptoms appeared: difficulty of walking and of standing, prostration, fallen countenance, dilated pupils, violent headach, white moist tongue; frequent, small, cordy pulse, hurried breathing, no apparent bubo, but great pain on motion of the right groin. 22nd. Bubo observable

very painful ; increase of all the symptoms. 23rd. Partial remission ; sinking in of the largest bubo. 24th. Breathing and pulse less frequent, and fallen ; skin moist ; tongue less white ; bubo less manifest than yesterday, strong feeling of hunger. 25th. Better ; very little pain in the bubo, and only when he moved ; recovery.

3rd. On the 23rd and 30th of August, Mahomet Halil, a criminal, was inoculated with the blood of a plague patient ; the first time, by means of four incisions in the fold of the left arm, and the right groin ; the second time, on the same places, on the opposite side. No other redness or inflammation than what was consequent on the mechanical injury supervened, and no general symptom appeared.

4th. A day later the inoculation was repeated on this man, for the third time, under the right arm-pit, and in the groin, with the matter of bubo. The evidence, as before, was negative.

5th. Mohiamed-Ebn-Ali, who had been dismissed cured, from the experiment with the clothes, twenty days previous, was now inoculated with the serosity taken from the broken phlyctena of a carbuncle. This third trial shewed just as little absorption as the former ; eight days later the blood inoculation was repeated also without success.

Next, M. Clot inoculated himself in the axilla and groin, with blood, without any consequences.

Such were the results of inoculation on five different persons, only one showed symptoms of plague ; but supposing that they had occurred in all, what conclusion could be drawn ? Would any one then be justified in assuming that plague was contagious ; or still more, that the inoculated matter carried the poison ? Certainly not, for all the other conditions were wanting. For it would be quite impossible to obtain any result under the circumstances in which it was here attempted, at the moment when plague was at its greatest intensity, in the midst of places it had desolated, in an hospital containing five to six hundred patients, and in a ward holding from fifty to sixty persons, who, for eight days, had acted as attendants on the sick, and who were exposed to all the conflicting influences which contagion, infection, atmosphere, and inoculation, could produce, and thus the materials here collected have no value either in pathology or legislation.

As general conclusions, we may lay down,

1st. That the existence of a contagious principle in plague has yet to be proved material.

2nd. That the positive and authentic facts which we have adduced in favour of immediate contagion, cannot be set aside

but that they must be verified by other not less authentic facts, in order to found a scientific principle.

3rd. That only some of these facts, brought forward by us to prove the possibility of mediate contagion, can be admitted, because the symptoms might arise from other causes.

4th. That the experiments, by inoculation on animals and healthy men, are still less important, both on account of the results being negative, and the conditions of time and place, with regard to the inoculated matter, and that for logical proof other experiments are still necessary.

6th. It will be necessary to institute fresh experiments during the plague period, on individuals out of the circle in which it is acting, and who, besides, have had no intercourse whatever with the diseased.

7th. That thus, all other causes being removed, such persons should be exposed to contact with plague subjects, without clothes. Others, similarly circumstanced, to the clothes alone, and thus clear proofs could be drawn.

8th. That the same rule should be followed in inoculating, that of conducting it out of the sphere of any other possible predisposing cause, and that blood, the matter of buboes, and the serosity of carbuncles, should again be tried, and in addition, that perspiration, saliva, bronchial mucus, and the matter of the first absorbent vessels, should also be severally inoculated.

Prophylactics.—As every circumstance tends to prove that virulent plague is incurable, at least in twenty cases in a hundred, and that it is only in the first stage that any treatment is of use, as well as that nature alone does much in all other cases, art is more called upon to guard against it than to cure it.

As Dr. Bulard has not, in his communication, spoken very decidedly on the Prophylactics of Plague, I think it better to omit what he has said, and I do so with the less regret, as this spirited gentleman has promised to write a monograph on this subject. We will only mention that, amongst other points, he states that vaccine virus has been supposed to protect against plague, yet, notwithstanding the assurances of M. Pariset, he considers it very far from proved; still he acknowledges that many physicians in the Levant believe it to possess this power.

In conclusion, the author states his intention of going to Constantinople, and determining there, if possible, the therapeutics, capability of infection of plague, its forms, effects, duration, and pathology, and by experiments, to endeavour to arrive at some plan of treatment, as well as some means of prevention.

We wish this bold physician every success in the arduous and perilous task which he is about to undertake in Asia Minor,

from which we hope he will escape, to reap the reward which grateful science will undoubtedly pour, with liberal hand, on her undaunted son.

S. L. L. BIGGER.

Experiments and Observations on the Gastric Juice, and the Physiology of Digestion.—By WM. BEAUMONT, M. D., &c. Reprinted from the Plattsburgh Edition, by ANDREW COMBE, M. D., Fellow of the Royal College of Physicians of Edinburgh, &c. Edinburgh, 1836.

THIS work, published in 1833, was first made known to European readers by an analysis of its contents in the *American Journal of the Medical Sciences*, which was transferred to the pages of the *Medico-Chirurgical Review*, and subsequently to those of the *Dublin Journal*; see vol. viii. At that time we were not in possession of any copy of the work. For the present edition, we are indebted to Dr. Combe, who, in a very well written preface, explains his reasons, both public and private, for putting the British public in possession of Dr. Beaumont's work.

Those who have not yet studied these experiments, must be informed, that they are twofold, having reference, on the one hand, to the phenomena of digestion within the body, and on the other to the action of the gastric juice on alimentary substance in external vessels. The opportunity of performing these experiments was afforded to Dr. Beaumont, in consequence of his having to treat a lad, named St. Martin, who recovered from a gun-shot wound of the side, with a fistulous opening into the stomach, closed by a sort of valve formed of a vascular structure, which being opened, permitted the introduction of foreign bodies directly into the ventricular cavity. From being a patient, this individual became a servant of the Doctor's, and submitted to a course of experiments, to which he could only have been reconciled by the very scientific diet with which he was supplied; soured tripe, whipped eggs, venison, wild turkey, sucking pig, wild duck, pancakes, oysters, sausages, fricassee of chicken, sponge cake, form but a small part of the bill of fare, to which we shall hereafter return, and indeed always think on with pleasure.

We cannot help declaring it as our opinion, that this work has been over praised. We give the author the full credit of industry, and a desire for truth; but we think his mode of ex-

perimenting very defective, at least for a basis of final conclusions on the physiology of digestion. Many of his deductions, too, are founded on an insufficient experience, even of the imperfect method which he followed, and we read of many singular circumstances, which, if established, would go far in modifying our pathological notions, yet which are passed by with a bare notice. Thus, we read of the stomach being covered, in a very complicated experiment, with numerous white spots, or pustules, resembling coagulated lymph, which were removed by a brisk calomel purge; what an important fact for our Abernethian, hepatic, chylopoietic, viscera-correcting practitioners.

One of the most interesting facts in the whole work, is the formation of a valve, by which the orifice of the stomach was kept closed.

“By the 6th of June, 1823, one year from the time of the accident, the injured parts were all sound, and firmly cicatrized, with the exception of the aperture in the stomach and side. This continued much in the same situation as it was six weeks after the wound was received. The perforation was about two and a half inches in circumference, and the food and drinks constantly exuded, unless prevented by a tent, compress and bandage.

“From this time he continued gradually to improve in health and strength, and the newly formed integuments over the wound became firmer and firmer. At the point where the lacerated edges of the muscular coat of the stomach and intercostal muscles met and united with the cutis vera, the *cuticle* of the external surface and the *mucous membrane* of the stomach approached each other very nearly. They did not unite, like those of the lips, nose, &c., but left an intermediate marginal space of appreciable breadth, completely surrounding the aperture. This space is about a line wide; and the cutis and nervous papillæ are unprotected, as sensible and irritable as a blistered surface abraded of the cuticle. This condition of the aperture still continues, and constitutes the principal and almost only cause of pain or distress experienced from the continuance of the aperture, the introduction of instruments, &c. in the experiments, or the exudation of fluids from the gastric cavity.

“Frequent dressings with soft compresses and bandages were necessarily applied, to relieve his suffering and retain his food and drinks, until the winter of 1823-4. At this time, a small fold or doubling of the coats of the stomach appeared, forming at the superior margin of the orifice, slightly protruding, and increasing till it filled the aperture, so as to supersede the necessity for the compress and bandage for retaining the contents of the stomach. This valvular formation adapted itself to the accidental orifice, so as completely to prevent the efflux of the gastric contents when the stomach was full, but was easily depressed with the finger.”—pp. 15, 16.

After St. Martin's recovery, which was in the spring of 1824; Dr. Beaumont waited for a year, and then commenced his "gastric experiments," at Fort Mackinnac, Michigan Territory. He subsequently pursued them at Burlington, Vermont, Plattsburgh, and at which place St. Martin disappeared; *abijt, evasit, erupit*. He returned to Canada, his native place, without, as Dr. Beaumont quaintly observes, "obtaining my consent."

But a man, with a hole in his stomach, was a prize not to be lost in this way. The patient was missing for four years; during which time he had married, become the father of two children, and enjoyed robust health. In 1825 he engaged as a *voyageur* with the Hudson Bay Company, and returned in 1826, and for the following year laboured hard to support his family.

"Accidentally learning about this time where he was, and that he enjoyed perfect health, I made arrangements with the agents of the American Fur Company, who annually visit Canada for the purpose of procuring voyageurs, to find and engage him for my service, if practicable. After considerable difficulty, and at great expence to me, they succeeded in engaging him, and transported him from Lower Canada with his wife and two children, to me, at Fort Crawford, Prairie du Chien, Upper Mississippi, a distance of nearly two thousand miles, in August, 1829. His stomach and side were in a similar condition as when he left me in 1825. The aperture was open, and his health good."—pp. 18, 19.

"He now entered my service, and I commenced another series of experiments on the stomach and gastric fluids, and continued them, interruptedly, until March, 1831. During this time, in the intervals of experimenting, he performed all the duties of a common servant, chopping wood, carrying burthens, &c. with little or no suffering or inconvenience from his wound. He laboured constantly, became the father of more children, and enjoyed as good health and as much vigour as men in general. He subsisted on crude food, in abundant quantities, except when on prescribed diet, for particular experimental purposes, and under special observance.

"In the spring of 1831, circumstances made it expedient for him to return with his family from Prairie du Chien to Lower Canada again. I relinquished his engagements to me for the time, on a promise that he would return when required, and gave him an outfit for himself, wife and children. They started in an open canoe, *via* the Mississippi, passing by St. Louis, Mo.; ascended the Ohio river; then crossed the state of Ohio, to the Lakes; and descended the Erie, Ontario, and the river St. Lawrence, to Montreal, where they arrived in June. He remained in Canada with his family until October, 1832, in good health and at hard labour. He was in the

midst of the cholera epidemic, at the time it prevailed, and passed through Canada, and withstood its ravages with impunity, while hundreds around him fell sacrifices to its fatal influence.

“In November, 1832, he again engaged himself to me for twelve months, for the express purpose of submitting to another series of experiments. He joined me at Plattsburgh, N. Y., and travelled with me to the city of Washington, where, with the facilities afforded by the head of the Medical Department, the experiments were continued upon him, from November, 1832, to March, 1833.

“During the whole of these periods, from the spring of 1824 to the present time, he has enjoyed *general* good health, and perhaps suffered much less predisposition to disease than is common to men of his age and circumstances in life. He has been active, athletic, and vigorous; exercising, eating, and drinking, like other healthy and active people. For the last four months, he has been unusually plethoric and robust, though constantly subjected to a continued series of experiments on the interior of the stomach; allowing to be introduced or taken out at the aperture different kinds of food, drinks, elastic catheters, thermometer tubes, gastric juice, chyme, &c., almost daily, and sometimes hourly.

“Such have been this man’s condition and circumstances for several years past; and he now enjoys the most perfect health and constitutional soundness, with every function of the system in full force and vigour.”—pp. 20, 21.

Before entering on the experiments, we must insert the following interesting account of the valve and appearance through the opening of the stomach:

“*Valve.*—The valve mentioned above, is formed by a slightly inverted portion of the inner coats of the stomach, fitted exactly to fill the aperture. Its principal and most external attachment is at the upper and posterior edge of the opening. Its free portion hangs pendulous, and fills the aperture when the stomach is full, and plays up and down, simultaneously with the respiratory muscles, when empty.

“On pressing down the valve when the stomach is full, the contents flow out copiously. When the stomach is nearly empty, and quiescent, the interior of the cavity may be examined to the depth of five or six inches, if kept distended by artificial means; and the food and drinks may be seen entering it, if swallowed at this time, through the ring of the œsophagus. The perforation through the walls of the stomach is about three inches to the left of the cardia near the left superior termination of the great curvature. When entirely empty, the stomach contracts upon itself, and sometimes forces the valve through the orifice, together with an additional portion of the mucous membrane, which becomes completely inverted, and forms a tumour as large as a hen’s egg. After lying on the left side, and sleeping a few hours, a still larger portion protrudes, and

spreads out over the external integuments, five or six inches in circumference, fairly exhibiting the natural rugæ, villous membrane, and mucous coat, lining the gastric cavity. This appearance is almost invariably exhibited in the morning, before rising from his bed.”—p. 23.

Dr. Beaumont's work may be divided into two parts, the first of which is a sort of essay on the subject of digestion generally, in which there is little new, and if we are to conclude that this essay is the result of the numerous experiments which follow in the second part, we must consider them as unnecessary, for really there is little here that we did not know before. Thus it is stated, that if we dine on venison steak and fat pork, a very Gothic and improbable combination, the time of digestion will be, in all probability, twice as long as if the venison was taken alone. Again, that oily substances are digested with great difficulty; that chymification is most readily effected on a soft solid; that tender meats are more digestible than when recent and resisting; that crude vegetables are not wholesome, &c. &c. &c. In this part of the work too, we object to the extreme looseness of expression with which it abounds; salt and vinegar are admitted as wholesome condiments:

“They both assist in digestion, vinegar rendering muscular fibre more tender, and both together, by producing a fluid having some *analogy to the gastric juice.*”

The above is an example out of many. Doctor Beaumont, on the other hand, condemns, unequivocally, *all spices*. Malt and vinous liquors, and even tea and coffee, do not escape his censure; here, however, his Editor reproves him.

“Agreeing with the author in the general opinions expressed in the text, I must differ from him as to the soundness of the argument founded on strong tea and coffee. His statement merely proves that *too much* is bad. Beef and mutton are in themselves very good, but too much of them is hurtful. Are we therefore to proscribe them? Weak tea *may* be good, although “*very strong*” tea is pernicious. I concur, however, in thinking, that tea, coffee, and stimulants are grossly abused.”—p. 41.

We think that Dr. Combe, in doubting “*the argument founded on strong tea and coffee,*” gives up the whole case; for it must appear, whatever Dr. Beaumont has written, (*for he has not* proved any thing to the contrary,) that a moderate use of wine may be as good as that of tea or coffee, and nothing that we have read can diminish our respect for Mr. Justice Inglewood, who could not have benefit from his victuals unless he was allowed “two hours of quiet leisure intermixed with harmless

mirth and a moderate circulation of the bottle.” Indeed, the author ought to have made some more researches on this point, before he condemned, so severely, the gladdener of the heart of man; and it is a fact, that, as far as the juice was concerned, it made no objection whatever to either wine or spirits, as we learn from the sixteenth and seventeenth experiments:

“EXPERIMENT 16.

“March 6.—At 8 o’clock, A. M., extracted two ounces gastric juice, and added it to two ounces of Madeira wine. No visible change was produced; no coagulæ formed. They united like pure water and wine. Heat produced no other effect.

“EXPERIMENT 17.

“March 7.—At 6 o’clock, P. M., stomach empty; extracted one and a half ounces of juice, and mixed it with the same quantity of Jamaica spirits. Effect same as with wine.”—p. 129.

Dr. Beaumont declares that nothing more than water is necessary for drink; that any thing more, indeed, is hurtful; and the accounts of the morbid appearances of the stomach after the use of ardent spirits, are held by Dr. Combe to have the greatest importance. But the case is not made out. We read, that,

“*St. Martin has been drinking ardent spirits pretty freely for eight or ten days past, he complains of no pain, nor shows symptoms of any general indisposition; says he feels well and has a good appetite.*”

There was some erythema, with apthous patches on the mucous surface; similar appearances are frequently described, and Dr. Beaumont and his Editor endeavour to impress on their readers, that ardent spirits are the principal exciting causes of these appearances, although the very same conditions were induced by the swallowing of food improperly masticated, or by the irritation produced by the scarcely justifiable experiments to which this victim of physiological curiosity was subjected. Dr. Beaumont, after observing on the fact of these appearances not being accompanied with any corresponding functional lesion, deduces from it, (by what process we cannot discover,) that *it is possible that there are good grounds* for the opinion of a celebrated teacher of medicine, “that most febrile complaints are the effects of gastric and enteric inflammations.” On this, we shall only observe, that the author’s pathological knowledge and reasoning powers seem clearly in harmony.

But we must glance at some of the experiments:

“EXPERIMENT 1.

“Aug. 1, 1825.—At 12 o’clock, A. M., I introduced through the perforation into the stomach, the following articles of diet, suspended

by a silk string, and fastened at proper distances, so as to pass in without pain, viz. : a piece of high-seasoned *à la mode beef*, a piece of *raw salted fat pork*, a piece of *raw salted lean beef*, a piece of *boiled salted beef*, a piece of *stale bread*, and a bunch of *raw sliced cabbage*; each piece weighing about two drachms; the lad continuing his usual employment about the house.

“ At 1 o'clock, P. M., withdrew and examined them; found the *cabbage* and *bread* about half digested; the pieces of *meat* unchanged. Returned them into the stomach.

“ At 2 o'clock, P. M., withdrew them again; found the *cabbage*, *bread*, *pork*, and *boiled beef*, all cleanly digested, and gone from the string; the other pieces of meat but very little affected. Returned them into the stomach again.

“ At 2 o'clock, P. M., examined again; found the *a-la-mode beef* partly digested; the *raw beef* was slightly macerated on the surface, but its general texture was firm and entire. The smell and taste of the fluids of the stomach were slightly rancid; and the boy complained of some pain and uneasiness at the breast. Returned them again.

“ The lad complaining of considerable distress and uneasiness at the stomach, general debility and lassitude, with some pain in his head, I withdrew the string, and found the remaining portions of aliment nearly in the same condition as when last examined; the fluid more rancid and sharp. The boy still complaining, I did not return them any more.

“ August 2.—The distress at the stomach and pain of the head continuing, accompanied with costiveness, a depressed pulse, dry skin, coated tongue and numerous white spots, or pustules, resembling coagulated lymph, spread over the inner surface of the stomach, I thought it advisable to give medicine; and, accordingly, dropped into the stomach, through the aperture, half a dozen *calomel pills*, four or five grains each; which in about three hours had a thorough cathartic effect, and removed all the foregoing symptoms, and the diseased appearance of the inner coat of the stomach. The effect of the medicine was the same as when administered in the usual way, by the mouth and œsophagus, except the nausea commonly occasioned by swallowing pills.”—pp. 117-119.

Here we have, at all events, a new cure for dyspepsia—*half a dozen calomel pills, four or five grains each !!* But the whole experiment is a burlesque on scientific investigation.

But again,

“ EXPERIMENT 29.

“ March 6. At 9 o'clock, A. M.; breakfasted on *venison steak*, *cranberry jelly* and *bread*, and drank a pint of *coffee*. Twenty minutes after eating, I took a portion from the stomach, in an incipient stage of digestion. Placed this on the bath.

“ At 9 o'clock, 45 mins.—I took another portion, in an advanced state of digestion ; very few small particles of food were discernible. At 10 o'clock, 10 mins. ; took out another portion, perfectly chymified. At 10 o'clock, 35 mins. ; the stomach was entirely empty and clean ; no chyme or aliment to be found in it. The breakfast, eaten at 9 o'clock, was all digested, and had passed through the pylorus, in *one hour and thirty-five minutes*.”—pp. 141-2.

This is a good illustration of his exceedingly loose style of experimenting. The patient gets, it must be admitted, an excellent breakfast, but in twenty minutes some of it is extracted, we are not told how much, nor whether it was put in again, after getting a warm bath. At nine o'clock, “ *another portion* ” is removed, and at ten, another. At half-past ten, the stomach is found “ empty,” and the experiment is quoted to prove the rapidity of digestion ; and Dr. Beaumont informs us, that “ *from various trials he is confident, generally speaking,* ” that venison is the most digestible diet.

The author wishes to determine the digestibility of fruit. He takes two grapes, the one *skinned*, the other not ; he places them in six drachms of gastric juice, and *keeps them in the axilla* for twelve hours. No change is produced. A fresh charge of an ounce of gastric juice is added, and the whole kept in for twenty-four hours, unaltered. In three days they began to smell badly, and this, he thinks, “ is a fair specimen of the indigestible nature of this kind of fruit.” Can absurdity go farther than this ? We shall not follow Dr. Beaumont further in his experiments ; but, before concluding this notice, we must observe, that a principal object of our author is to defend that theory of digestion which refers the changes of aliment in the stomach to a purely chemical process. We have always considered this doctrine as a very doubtful one, inasmuch as it seems greatly at variance with our more extended notions of physiology.

Those who maintain, like Dr. Beaumont, that the gastric juice is the sole agent of ventricular digestion, assume the existence of a combination possessing an extent of solvent powers for which there is neither proof nor analogy. This gastric juice, this general solvent, when examined, contains a little free acid, and some salts in a state of great dilution. Yet, according to the school of chemical physiologists, it is capable of dissolving an almost infinite variety of substances or combinations of substances. When we consider the extent of the catalogue of alimentary materials used by man, from the complicated dishes of the modern epicure to the clay balls of the Ottomacs, a catalogue including almost every article of organic, and many of inorganic product, the idea that *one solvent* could dissolve all

appears utterly impossible. But it may be contended, that the composition of the gastric juice may vary. We answer, that there is no proof, nor shadow of a proof, that it varies according to the tribe or race of man ; and if it varies according to the *aliment*, then the whole case is given up. We must then grant to the stomach a discriminating power, and accord it higher properties than a vessel for a chemical solution in a fluid whose properties are constant and determinable by analysis.

Dr. Beaumont uses the term saturation in a mode which is new to us. He speaks of the gastric juice being *saturated*, *refusing to dissolve* more, &c. Now, if he means, as we must suppose him to do, that there is an analogy between such an occurrence and the saturation of an acid or an alkali, or any other chemical combination, we must wholly dissent from such a view. He supposes a certain quantity of gastric juice *secreted*, or ready to be secreted. This is the first assumption ; and next he wishes to prove, that a combination between a certain quantity of a fluid which we cannot consider otherwise than a mere mixture, and elements infinitely various in quality and quantity, is analogous to the saturation of an acid with a certain quantity of a base, or *vice versa*.

And without denying the action of a gastric juice, there is much reason to believe that it varies. The effects of stimulants on the living organism vary in a two-fold manner ; first, according to the state of innervation of the part ; and secondly, according to the degree or the mode of stimulation. One stimulant will produce a discharge of serum, another of pus ; and is it not likely that the secretions of the stomach vary in a similar manner ; vary, we would say, with the stimulant employed.

But this leads to a still more important consideration : if there be this variety in secretion corresponding to the variety in aliment, the first step in digestion must occur where the alimentary molecule is in contact with the living tissue. This we believe to be the first point of digestion, the first step in the wonderful and still unintelligible process by which the property of life is communicated to what has no life. Without denying the occurrence of chemical actions, we believe that digestion is as much a vital process as respiration, secretion, or any other of the phenomena of the living body. The progress of physiology is towards vitalism, a progress ever opposed by the vain and uninformed ; and nothing that Dr. Beaumont has written or observed, is calculated to shake our opinion, that the chemical theory of digestion is quite insufficient to explain the mysteries of this process.

INDEX

TO THE THIRTEENTH VOLUME.

A.

	Page.
Acid of Haller, external application of	143
Acetabulum, perforation of, by a fall on the trochanter	155
Anatomy of Cowper's Glands in the female, by J. R. TAYLOR, Surgeon, 58th Regt.	104
Anatomy and Physiology, Cyclopedia of, by Doctor ROBERT TODD. <i>Review</i>	159
Asthmatic, Journal of an	23

B.

Bagnères and Barége, waters of	286
Baldness, Pomade to cure	151
Belladonna, on the use of, in certain cases of fever, by Doctor GRAVES	351
BIGGER's, Doctor, gleanings	138
BLACKLEY's, Mr. observations on cynanche trachealis and laryngitis	384
BREWSTER's, Sir DAVID, theory of cataracts. <i>Review</i>	384

C.

Callanan, Doctor Albert H., on the pathology and treatment of typhus fever. <i>Review</i>	320
CARMICHAEL, Mr., views with respect to the use of mercury in the venereal disease	118
on sciatica, and other neuralgic affections, &c. &c.	286
Mr. Andrew, letter to the Editors, by	250
Cataracts, review of Sir David Brewster's theory of	315
Churchill, Fleetwood, M.D., on diseases of females. <i>Re- view</i>	162

	Page.
Churchill, Fleetwood, M. D., Report of Western Lying-in Hospital	233
Cirrhosis of the lung, by Doctor CORRIGAN	266
COLLINS, Doctor, observations on Professor Hamilton's deviations from the ordinary modes of stating practical results	402
CORRIGAN, Doctor, on cirrhosis of the lung	266
Corrosive sublimate in syphilitic pains	148
Cove, medical topography of, by Doctor DAVID H. SCOTT	55
Cowper's glands in the female, anatomy of, by Assistant Surgeon J. R. TAYLOR, 58th Regt.	104
Crystalline lens, ossification of	156
Cynanche trachealis, on the diagnosis of, by Travers R. Blackley, A.B. M.R.I.A.	384

D.

Delirium tremens	152
----------------------------	-----

E.

Elliotson, Dr., letter on the metaphysical and phrenological opinions of	169
Epiglottitis, on acute inflammation of the, by DR. MARSH	1
Epilepsy cured by indigo	138
Eye, five iron splinters removed from	153

F.

Fever, some observations on, by HENRY KENNEDY, A.B.	107
---	-----

G.

Gastric juice, experiments and observations on the, and the physiology of digestion. <i>Review</i>	500
Gout in the penis	138
GRAVES, DR. ROBERT J., on the state of the pupil in typhus, and the use of belladonna in fever	351

H.

Hæmorrhage, secale cornutum in	148
HAMILTON, JOHN, L.R.C.S., on some effects resulting from wounds of nerves	38
PROFESSOR, letter from, to Dr. Collins	201
Heart, nitrate of silver in spasmodic affections of	148
Hydrargyrum iodicum in syphilis	148

I.

	Page.
Incontinence of urine, cured by strychnine	145
Indigo, a cure for epilepsy	138
Induration of the skin	158
Inflammation, acute, of the epiglottis, by Dr. Marsh	1
Inflammation of the testicle treated by compression	146
Iron splinters, removal of, from the eye	153

K.

KENNEDY'S, Dr. HENRY, observations on fever	107
Kidneys and epidermoid glands, identity of structure between, by ROBERT ELLIOTT LINDSAY	395

L.

Laryngitis, on, by Dr. Blackley	384
Lens, crystalline, ossification of	156
Letter on the metaphysical and phrenological opinions of Dr. Elliotson	169
Lindsay, Mr., on the identity of structure between the kidneys and epidermoid glands	395
Lung, on cirrhosis of the, by Dr. Corrigan	266
Lying-in Hospital, &c., second report of, by Fleetwood Churchill, M. D.	233

M.

MARSH, Dr., on acute inflammation confined to the epi- glottis	1
Medical topography of Cove, by Dr. DAVID H. SCOTT	55
Medical profession, on the nature and cultivation of the, by George Morgan, A. M. <i>Review</i>	166
MOLLAN, Dr. JOHN, statistical report of the Richmond Lunatic Asylum	367
Morrison's, Dr. J., report of the Newry Dispensary and Fever Hospital for 1837	249

N.

Nerves, on some effects resulting from wounds of, by JOHN HAMILTON, L. R. C. S.	38
Neuralgia, oil of turpentine in,	147
Newry Dispensary and Fever Hospital, report for 1837, by Dr. Morrison	249
Nitrate of silver in nervous diseases of the stomach	140
in spasmodic affections of the heart	148
Numerical tables on the influence of climate on phthisis	156

O.

	Page.
Oil of turpentine in neuralgia	147
Ossification of the crystalline lens	156
Outlines of the principal diseases of females &c. by Dr. Churchill. <i>Review</i>	162
Ovaries from cows, effects of removal of	159

P.

Partus per anum	140
Pathological signs, plan of. <i>Review</i>	161
Pathology and treatment of typhous fever, by Dr. Callanan. <i>Review</i>	320
Penis, gout in the	138
Perforation of the acetabulum caused by a fall on the trochanter	155
Philadelphia Medical Examiner	349
Physiology of digestion, &c. <i>Review</i>	500
Plague, from actual observation, by Dr. Bulard, (de Meru.) <i>Review</i>	459
Pomade to cure baldness	151
Pupil, the state of, in typhus by Dr. Graves	351

R.

Rabies canina communicated from one human being to another	142
Radical cure of varicose veins	157
Richmond Lunatic Asylum, statistical report of, by Dr. Mollan	367

S.

Sciatica and other neuralgic affections, &c. &c. observations on, by R. CARMICHAEL, M.R.I.A.	286
SCOTT, DR. DAVID, medical topography of Cove by,	55
cases by	442
Secale cornutum in hæmorrhage	149
Skin, general induration of the	158
Spasmodic affections of the heart, nitrate of silver in	148
Splinters of iron, removal of, from the eye	153
STABERON, DR. JULIUS, on typhous fever	426
Stomach, nervous diseases of, cured by nitrate of silver in its morbid states, by Langston Parker, Esq. <i>Review</i>	140
Strychnine, a cure for incontinence of urine	334
Syphilitic pains in the bones, corrosive sublimate in	145
buboes, treatment of by seton	148
swellings, hydrargyrum iodinicum in	155
ulcers, practice of Ricord in	148
	153

T.

	Page.
Tinea capitis	153
Turpentine, oil of, in neuralgia	147
Typhus, on the state of the pupil in, and the use of belladonna in certain cases of fever, by DR. GRAVES	351
Typhus, on occurrence of, in manufacturing cities, and its enteric complication, by DR. STABEROH OF BERLIN	426
Urine, incontinence of, cured by strychnine	145
Varicose veins of the leg, proximate cause and radical cure of	157
Venereal disease, summary of MR. CARMICHAEL'S views of the use of mercury in	118
Wounds of nerves, on some effect of, by JOHN HAMILTON, L.R.C.S.	38

END OF VOL. XIII.





